SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

FRIDAY, MAY 4, 1906.

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THE WORK OF HUGO DE VRIES AND ITS IMPORTANCE IN THE STUDY OF PROBLEMS OF EVOLUTION.[1]

As Professor Osborn, in his 'From the Greeks to Darwin,' has given an account of the development of the theory of evolution in Europe, there is no necessity for me to repeat its history. However, I should like to remark in passing, that the speculations of the oriental philosophers, especially the early Hindoos, have not received in the occident the attention due them, and to express the hope that some scholar will present to us of the occident the results of the thinking of the eastern sages on these problems.

That existing species of animals and plants have been derived from preceding species by natural processes is now universally believed by biologists. The man most potent in establishing this belief on a firm foundation was Darwin. Here it is unnecessary to do more than allude to his doctrine of the struggle for existence and the survival of the fittest by means of natural selection. Although I shall not present the data that he accumulated, I desire

[1] An address delivered before the Biological Society of Washington, on February 3, 1906.

especially to call your attention to the great number of instances of the utility of structures and adaptation to environment adduced by him. There is a mass of evidence showing that organisms possess useful structures and are more or less in harmony with their environment. I, therefore, assume both the theory of evolution and that organisms are more or less perfectly adapted to their environment.

There are three hypotheses to account for the evolution of new species and their perpetuation. Each one of the hypotheses recognizes the struggle for existence and the influence of natural selection.

The first hypothesis is considered peculiarly the Darwinian, to which Weismann has added his theoretical supplement. According to this hypothesis, animals and plants are subject to fluctuating variations of small amount. Those individuals whose variation gives them an advantage over their competitors for life are preserved, producing a gradual amelioration. This process is continued until ultimately the descendants differ so widely from their progenitors that they are referred to a separate species. The Weismannian supplement denied the hereditary transmissibility of characters impressed on the individual by its environment, so that the adjustment of organisms to their environment would depend entirely upon the preservation of favorable fortuitous variations.

The second hypothesis, usually known as the neo-Lamarckian, called the dynamic by Dall, and that of direct causation by Nägeli, for many years the rival of the one just stated, undertakes to account for the phenomena of adaptation by assuming that the organisms are directly molded into harmony with their environment by external forces, or that adaptation may originate through conscious effort, and that changes in individuals so brought about are transmitted to their offspring. The principal defenders of this hypothesis in this country were Cope and Hyatt. Dall, although he proposed the term dynamic, I believe, never accepted Cope's hypothesis of archæsthetism, an idea first advanced by Lamarck.

The third hypothesis is that of de Vries. He admits the struggle for existence, and recognizes fluctuating variation. He, however, contends that this kind of variation will not give rise to a new species, basing this conclusion on a large amount of experimental data obtained from the breeding of plants. According to him all that can be done by selecting the best individuals is to ameliorate the race to a certain point, beyond which no progress can be made, and that so soon as the process of continued selection is abated, regression toward the average variation of the species or variety ensues. De Vries also denies the permanency of the effect of natural selection on the fluctuating variations of a species. Natural selection, he claims, can not accumulate variations of this type beyond a certain limit; and that if individuals of such a naturally ameliorated race be transferred to another area they will regress toward the mean characters of the species. Natural selection has preserved certain individuals of the species, but has not really changed it. From a study of the mode of the appearance of the permanent 'varieties' or 'species' of cultivated plants, de Vries was led to believe that new species do not originate either by the gradual elimination of unfavorable fluctuating variation or by the direct modifying influence of environment, but by sudden mutation—the offspring differing from their parents by distinct lacunæ, breeding true and showing no tendency to revert to the parental form. His acquaintance with cultivated plants

led him, as was stated, to this conclusion. He then conceived the idea of searching in nature for a species in a state of mutation. The story of his discovery of mutating individuals of Lamarck's evening primrose, *Œnothera lamarckiana,* in the vicinity of Hilversum, Holland, need not be repeated. From specimens of this species he obtained by pedigree cultures seven new forms, six of which he has described as new species and one as a new variety. Besides this experimental evidence in favor of his hypothesis, de Vries compiled a large amount of data that give additional weight to his conclusion.

These are the three principal hypotheses attempting to account for the origin of new species. I think that it is not necessary to give a special discussion of isolation as a factor in evolution, as it does not affect the validity or invalidity of any one of the hypotheses stated. Isolation is a passive, not an active factor; its importance, however, is beyond question.

On the leaf following the title page of 'Species and Varieties, their Origin by Mutation,' are three quotations:

'The origin of species is a natural phenomenon.'—Lamarck.

'The origin of species is an object of inquiry.'—Darwin.

'The origin of species is an object of experimental investigation.'—De Vries.

The history of any movement of thought is always from a greater or less indefiniteness to greater definiteness and precision. The solution of any complicated problem must be preceded by the analytical work that discovers the factors involved.

The following will indicate the tendencies and stages of such a movement:

1. The accumulation of data, largely through mere curiosity.

2. An attempt to discover some causal relation underlying the phenomena, and the propounding of an hypothesis.

3. The energetic accumulation of additional data, especially to controvert or sustain the hypothesis already propounded.

4. A critical reexamination of the accumulated data to discover if they are susceptible of a different interpretation, or if there may not be some previously undiscovered underlying principle.

5. The announcement of a different interpretation or a new principle will produce additional activity both in the accumulation of data and in the attempts to interpret them.

6. There will be recurrent periods of the accumulation of facts with reference to old theories, and a continual critical reexamination both of facts and of theories. These will lead to a more highly developed critical faculty and more refined methods of research.

Previous to Lamarck there were a number of zoologists and botanists energetically recording their observations, especially as a result of the classification and system of nomenclature proposed by Linnæus. Lamarck proposed the theory of evolution that has been revived by the neo-Lamarckian school. Darwin correlated the data previously accumulated, supplemented by a stupendous number of observations of his own, and gave us his 'Origin of Species.' The accumulation of data has continued, and the critical examination of previously announced conclusions becomes more acute. With this more highly developed critical faculty have come additional methods of investigating the problems of evolution.

De Vries's book, 'Species and Varieties, their Origin by Mutation,' contains not only a vast body of highly important facts, but is pregnant from cover to cover with suggestions regarding important lines of

needed research. This volume is divided into six principal sections:

I. *The Introduction.*—This section contains a brief review of the leading theories of evolution and a statement of his own method of studying the problem.

II. *Elementary Species.*—According to de Vries the species of systematists are not single groups of individuals intergrading among themselves and separated by lacunæ from neighboring groups, but are aggregates of such units. He, therefore, speaks of 'systematic species' and 'elementary species,' an 'elementary species' being one of the units that go to make up the 'systematic species.' I think it very unfortunate that de Vries has introduced the term 'elementary species' as opposed to 'systematic species.' It clearly shows that he is not in touch with refined modern systematic work, for his 'elementary species' is the systematic species of all modern systematists with whose work I am familiar, while his 'systematic species' has no status. However, his account of how species established by the older systematists are now being subdivided into smaller and smaller units as our studies become more detailed and more exact, is very interesting. He discusses elementary species not only in nature, but also among cultivated plants, and gives valuable information regarding the mode of their selection among the latter, especially wheat.

III. *Retrograde Varieties.*—I think de Vries again unfortunate in his attempt to define a variety as an elementary species that has lost a character. According to the usually accepted definition of variety in this country there is intergradation with the typical form of the species: a variety would be represented by a secondary mode in the species curve. The inference from de Vries's definition would be that we can not have retrogressive species. It is well known in paleontology that not only single species, but whole groups of animals have undergone retrogressive evolution—the Baculites, for instance. In this section de Vries brings out a very important distinction between two classes of phenomena that have been characterized as atavism. Real atavism is defined as the reappearance of ancestral characters in a species that is pure bred. The other phenomenon, and the one that is usually designated atavism, is considered false atavism by him, and the name 'vicinism' applied to it. 'Vicinism' is due to the crossing of species or varieties. A plant the result of such a cross, though apparently a pure strain, may produce offspring of the type of the ancestor that it least resembles. This section contains much information on crosses, including a statement of Mendel's law.

IV. *Ever-sporting Varieties.*—The term 'ever-sporting variety' is used for those forms regularly propagated by seed, of pure, not hybrid, origin, but which 'sport in nearly every generation.' An interesting account is given of how he tried to obtain a pure striped and a pure red variety from a striped variety of snapdragon, known as *Antirrhinum majus luteum rubro-striatum,* which sports into red flowers, but he was not successful. The seed of red individuals, or even seed from self-pollinated red flowers in a raceme of mostly striped flowers, would produce a certain number of striped flowers. The seed from striped individuals always gave a percentage of red ones. It is interesting to note that stocks producing double flowers are grown from single-flowered individuals. These experiments are so interesting, and so suggestive of further research, that it is a great temptation to give a detailed account of them, and also to describe the experiments in trying to get a race of five-leaved clover, and those with polycephalic poppies and monstrosities. His experiments with double adaptations are

immediately germane to the subject under discussion, and they can be more fully described. But before proceeding to a discussion of them, the factors that lead to the phenomena of ever-sporting should be analyzed. According to de Vries,

> The wide range of variability of ever-sporting varieties is due to the presence of two antagonistic characters which can not be evolved at the same time and in the same organ, because they exclude one another. Whenever one is active, the other must be latent. But latency is not absolute inactivity and may often only operate to encumber the evolution of the antagonistic character, and to produce large numbers of lesser grades.

On a subsequent page, he says of the characters:

> They might be termed alternating, if it were only understood that the alternation may be complete or incomplete in all degrees. Complete alternation would result in the extremes, the incomplete condition in the intermediate states. In some cases, as with the stocks, the first prevails, while in other cases, as with the poppies, the very extremes are only rarely met with.

De Vries says:

> Taking such an alternation as a real character of the ever-sporting varieties, a wide range of analogous cases is at once revealed among the normal qualities of wild plants. Alternation is here almost universal. It is the capacity of young organs to develop in two divergent directions.

These phenomena are illustrated by numerous illustrations drawn from those presented by wild plants. The water-persicaria, *Polygonum amphibium,* is the one first cited. This plant occurs in two forms, one aquatic and the other terrestrial. The aquatic plants, known as var. *natans,* "have floating or submerged stems with oblong or elliptical leaves, which are glabrous and have long petioles. The terrestrial plants (known as var. *terrestre* or *terrestris*) are erect, nearly simple, more or less hispid throughout, with lanceolate leaves and short petioles, often nearly sessile." These "two varieties may often be seen to sport into one another. They are only branches of the same stem grown under different conditions."

Numerous other instances of double adaptation are given. Those taken from alpine plants transferred to the lowland are in some respects the most interesting for our discussion. De Vries says: "It is simply impossible to decide concerning the real relations between the alpine and lowland types without experiments." Some experiments are given by which the factor determining the change in character was discovered.

In concluding his remarks on these phenomena, the statement is made:

> Useful dimorphism or double adaptation, is a substitution of characters quite analogous to the useless dimorphism of cultivated ever-sporting varieties and the stray occurrences of hereditary monstrosities. The same laws and conditions prevail in both cases.

Interjected into this chapter is a consideration of the 'Theory of Direct Causation,' first advanced by Lamarck, subsequently advocated by Nägeli, von Wettstein, Strasburger and other German botanists, also by Hyatt, Cope and others in this country. The instances of double adaptation, of course, do not support this theory. De Vries gives an account of some plants that grow in the Desert of Kaits, Ceylon. These plants, although they have apparently grown in the desert for many centuries, have not become of the desert type, still possessing a thin epidermis and exposed stomata; and were shown by Holtermann to lose the only desert character that they had, their dwarf stature, when grown on ordinary garden soil. These plants disprove the Nägelian contentions: (1) That extreme conditions change organisms in a desirable direction; (2) that the only change induced by the dry soil, decreased stature, was not hereditary.

V. *Mutations.*—Under this heading are

included the chapters treating of de Vries's work on the production of the peloric toad flax, double flowers and new species and varieties of *Œnothera*. He also presents his views, with his seven laws, on 'the origin of wild species and varieties.' The last four chapters are entitled Mutations in Horticulture, Systematic Atavism, Taxonomic Anomalies and Periodic Mutations. Interesting data are presented in each one of these chapters. The phenomena of mutation in horticulture and taxonomic anomalies are in general in line with the general thesis being defended.

The idea that the species are constant through extended periods of time, following which is a period of mutation with the production of new forms, those best accommodated to the environment surviving, is important.

Dall proposed the term saltatory evolution in 1877, and suggested periodic mutation. His conclusions, however, did not rest on observed experiments.

VI. The last section of the book deals with 'fluctuations.' An account is given of the means of the statistical study of variation. Quetelet's law is stated, etc. In this section it is contended that fluctuating variation does not exceed certain definite limits, and that in cultivation, without the appearance of desirable mutations, it is not possible to ameliorate a species beyond a fixed degree. Ameliorated races, without continual selection, regress toward the mean of the species. In the last chapter of the book it is maintained that natural selection can do no more toward the creation of new species by an accumulation of fluctuating variation than can artificial selection.

From this very defective review of de Vries's work, it will be seen that he has investigated a wide range of phenomena. His method has mostly been by experiment; his results are such as to compel a critical reexamination of the views current on the process of evolution. I shall hastily criticize the three theories stated in the beginning of this discussion.

1. *The Darwinian Hypothesis.*—According to researches into the variability of organisms, fluctuating variation is around a mean and never transgresses certain limits. It is not possible to ameliorate a particular species beyond a fixed degree, and it is, therefore, impossible radically to change it. If continued artificial selection is not practised, the ameliorated race regresses toward the mean of the species. It is contended by de Vries that the same is as true of wild species as of those in cultivation. If the criticism can be sustained, this hypothesis must be abandoned.

2. *The Dynamical Hypothesis.*—Dall, in his paper entitled 'On Dynamic Influences in Evolution,' said:

> Passing from these general considerations to those of a more special character, the contention of Weismann that 'not a single fact hitherto brought forward can be accepted as proof' of the transmission of acquired characters demands attention.

In reply he says:

> If the dynamic evolutionist brings forward an hypothesis which explains the facts of nature without violence to sound reasoning, that hypothesis is entitled to respect and consideration until some better one is proposed or some vitiating error detected.

Some years ago, while a student at Harvard, I had the opportunity to attend a symposium on the hereditary transmission of acquired characters. The principals in the discussion were Professor Poulton, of Oxford, and Professor Alpheus Hyatt. The data presented by Professor Hyatt were subsequently published in his memoir entitled 'The Phylogeny of an Acquired Characteristic.' After the discussion, my conclusion was that Professor Poulton had shown that all experimental evidence was against the transmission of acquired characteristics; while Professor Hyatt pro-

duced no direct evidence to sustain his contention. What he did, was to show that a body of facts were in harmony with the *assumption* that acquired characteristics were inheritable, but his facts were as fully in accord with the *assumption* of Professor Poulton. As Professor Poulton had a certain amount of experimental evidence and Professor Hyatt had none, I thought that Professor Poulton had the better of the argument, but he did not show that acquired characters could not be inherited.

Since hearing the discussion between those eminent men, I have made an effort to go over the arguments for and against the 'Theory of Direct Causation.' I have read much or most of what Spencer, Cope, Hyatt and Dall have written on the subject. Dall has stated the proposition fairly and unequivocally. The facts that they present can be explained on their fundamental assumption, but they produce no direct evidence that that assumption is correct. Nägeli, von Wettstein and Strasburger represent in botany what Cope, Hyatt and Dall represent in zoology. As has already been stated, de Vries has collated a mass of evidence, all of which is against the views held by Nägeli among the botanists. The positive evidence is against the 'dynamic theory' or the 'theory of direct causation.'

But I wish to repeat the words of Dall:

> If the dynamical evolutionist brings forward an hypothesis which explains the facts of nature without violence to sound reasoning, that hypothesis is entitled to respect and consideration until some better one is proposed or some vitiating error detected.

It should also be remarked, that while Weismann denies the inheritance of functional variation, causing atrophy or hypertrophy of a part, he admits that climate may produce hereditary changes by acting on the germ-plasm. He, however, does not commit himself to the belief that such does occur.

I should like to enter into a full discussion of the effects of temperature on the pupæ of *Polyommatus phlæas* in influencing the color of the wings of the adult, but time does not permit. However, according to Weismann there is a critical period, after which the raising or lowering of the temperature does not affect the wing color of the adult. Weismann has pointed out that in southern Europe the golden-winged spring brood is derived from the pupæ of the dark-winged summer brood; while the dark-winged summer brood is derived from the golden-winged spring brood. An increase in temperature does affect the wing coloration, but there is no evidence to show that a permanent hereditary change is wrought. In fact the evidence is contrary to such an assumption. The period of sensitiveness to temperature in these butterflies is comparable to the period of sensitiveness discovered by de Vries in developing polycephalic poppies. Although the polycephalic poppies fluctuate in the number of converted stamens from almost 0 to 150 and over, de Vries found no instance of heads without indications of pistillody of the stamens, and in no instance were all the stamens transformed into pistils. The relative number of converted stamens, however, is largely determined by physical conditions; abundance of plant food and a sunny exposure are essential for the best results. The point of similarity between these experiments is that de Vries discovered in developing poppies and Weismann in developing *Polyommatus* a period of sensitiveness to external conditions. After this period is passed, varying physical conditions do not affect the fully developed adult.

Weismann's butterflies belong in de Vries's category of 'ever-sporting varie-

ties,' and are comparable to the water persicaria and polycephalic poppies.

Weismann speaks of Nägeli's experiments on *Hieracium*. He says:

> Many climatic varieties of plants may also be due wholly or in part to the simultaneous variation of corresponding determinants in some part of the soma and in the germ-plasm of the reproductive cells, and these variations must of necessity be hereditary. Temperature, and nutrition in its widest sense, affect the whole body of the plant —the somatic as well as the germ-cells.

De Vries shows that the species of *Hieracium* studied by Nägeli exhibit the phenomena of double adaptation. There is no evidence that this attribute is originated by the direct influence of physical environment.

Although we have ample grounds for doubting the validity of the assumption of the adherents to the dynamic theory, we can not yet refuse their hypothesis respectful consideration.

3. *The de Vries Mutation Hypothesis.*—This hypothesis rests upon a negative and a positive basis. The former is the negation of the abilit[illegible] the two preceding hypotheses to account for the origin of species, affirming that fluctuating variation is only between definite limits with reference to a mean, and that environment does not directly modify species. The positive element is the observation of new forms arising from older ones by mutation. Each of these conclusions of de Vries is open to challenge. (1) Have sufficient data been accumulated to justify our discarding the hypothesis that new species may originate by the gradual accumulation of variations that tend in a certain direction? (2) Is the evidence submitted sufficient to warrant the permanent rejection of the dynamic hypothesis? (3) Are his supposed mutations really mutations? The parentage of his *Œnothera lamarckiana* is not known. May not his new *Œnotheræ* be hybrids of some kind?

These different hypotheses present different explanations of phenomena assumed to be true by each one. I think that they render necessary a more critical analysis of the biological facts cited to substantiate each one.

At the last meeting of this society Dr. Merriam presented a paper, 'Is Mutation a Factor in Evolution?' His facts were that various regions are inhabited by subspecies of mammals or birds in accordance with their differences in physical conditions, and that the transition zone between two regions is occupied by intergrading forms. Take, for instance, two adjacent areas presenting different physico-geographic characters: one subspecies would be found in one area; in the other area, another subspecies. The physical conditions in going from one area to the other do not change abruptly, but gradually. The intermediate zone is not only intermediate in physical characters, but is occupied by individuals that are intermediate in their characters between the subspecies of the two different areas. As Dr. Merriam quoted the hammer and anvil simile of Dall, we are, I think, justified in placing him in the category of the dynamic evolutionists. His conclusions were:

1. There is evidence of the intergradation between species.

2. The direct influence of environment is the principal factor in the production of new species.

3. In the higher vertebrates there is no evidence of the origin of species by 'mutation.'

As to Dr. Merriam's statements regarding the classification and distribution of the animals discussed in his communication, we can say nothing, for all of us know of the many years that he has spent collecting and studying them and plotting their distribution with reference to geographic conditions. But I think his explanation of the phenomena open to question.

I will admit that Dr. Merriam's explanation of his facts may be true, but he did not convince me of its correctness any more than Professor Hyatt convinced me of the correctness of his interpretation in his 'Phylogeny of an Acquired Characteristic.' His facts seem just as plausibly explicable on the basis of the Darwinian hypothesis or that of de Vries. According to the former those variations tending to give the species an advantage in the struggle for its life would be preserved, while other variations would be eliminated. This preservation of certain individuals and the elimination of others would cause divergence in the characters of the occupants of the respective areas. In the intermediate zone, as there would not be definite selection, there would not be distinct differentiation of type.

The de Vries hypothesis will explain them just as well. The forms occupying the respective areas may have originated by mutation, and the intermediate zone may be occupied by hybrids.

The evidence in favor of none of these hypotheses is conclusive.

The facts presented by Dr. Merriam are a necessary foundation for the recognition of the factors involved in the problem, but they do not solve the problem. I should like to know:

1. Something concerning the stability of the characters of the forms inhabiting the different areas.

In this connection the following questions may be asked:

(*a*) Is the difference observed between the individuals occupying the different areas caused by the direct influence of physical environment? If the difference is caused by such influence, is the change so wrought only superficial or is it hereditarily transmissible? The feathers of birds exposed to strong sunlight are of lighter color than those of birds living in areas in which the light is weaker. From characters of this kind we can infer that the specimens exhibiting them lived under certain conditions. Do changes of this kind extend to the gametes of the individual or are they only somatic changes, enabling us to infer that an individual lived under certain physical conditions, similar to the inference drawn from seeing a man with certain scars on his face, viz., that he has attended a German university?

Dr. Dall, in his review of Gulick's 'Evolution, Racial and Habitudinal,' says concerning the theory of segregation advocated by that author:

> To justify final acceptation an hypothesis must not only be capable of accounting for the facts, but it must be shown to be the only one by which they may be adequately explained. It is also necessary to determine how far the animals in question have arrived at that state of equilibrium which we recognize by the name of species. If, as has been held by some authorities, the small color groups are really only of a temporary nature, and liable to immediate change upon subjection to modified environment, then the author's hypothesis, while losing none of its truth, is not a contribution to the evolution of species so much as to the physiology of color variation.

(*b*) Should the differences be gametic in origin, *i. e.*, not induced by the physical environment, is the selection between divergent variations of one species; or is it between two different species?

I could present a series of observed phenomena in the Madreporaria parallel to some of the data presented by Dr. Merriam in his discussion of the distribution of mammals and birds. These instances could be drawn from several genera, but those from *Turbinaria* are especially *à propos*. Mr. Pace has carefully studied these corals in the Torres Straits; he, however, discretely remarks:

> It will now be my endeavor to show that the variations of a turbinarian colony from the primitive cup-shape—the 'crateriform' type of Bernard—can be readily explained by reference to the

conditions under which the coral has grown; though it by no means follows that heredity plays no part in determining the form of the growth assumed by the corallum under any particular conditions, and it may well be that the *tendency* toward one type rather than another is inherited; *this, however, can only be established by experiment.*

I have italicized the words: 'this, however, can only be established by experiment.'

2. Are the intermediate specimens in the intermediate area actually intermediate in character or are they hybrids?

The following known occurrence of hybridization taken from de Vries suggests that a similar phenomenon might occur in the intermediate areas described by Dr. Merriam. *Rhododendron intermedium* is an intermediate form between the hairy and the rusty species from the Swiss Alps, *R. hirsutum* and *R. ferrugineum*, the former growing on chalky, and the other on siliceous soils. Whenever these types of soil occur in the same valley and these two species approach one another, the hybrid *R. intermedium* is produced, and is often seen to be propagating itself abundantly. As is indicated by the name, it combines the essential characters of both parents.

De Vries says:

It is not to be forgotten, however, that all taxonomic distinctions, which have not been confirmed by physiological tests are only provisional, a view acknowledged by the best systematists. Of course the description of newly discovered forms can not await the results of physiological inquiries, but it is absolutely impossible to reach definite conclusions on purely morphological evidence. This is well illustrated by the numerous discords of opinion of different authors on the systematic worth of many forms.

Until various physiological tests of the kind referred to by de Vries have been made, more than an hypothetical explanation of the facts presented by Dr. Merriam is impossible.

I now wish to reiterate my opinion as to the importance of the work of de Vries. The great value of his work consists in having shown that 'The origin of species is an object of experimental investigation,' and having furnished guidance not only as to *what* experiments should be made, but as to *how* they should be made.

Davenport in his last report to the president of the Carnegie Institution says:

The factors of evolution are three—variation, inheritance and adjustment. Studies may be made on any one of these factors or all three together; as a matter of fact, they can hardly be studied wholly independently.

The discussion to follow will cover in its range each of these factors.

As I have opened the discussion it might be expected that I should furnish specific data bearing upon these questions. I can furnish instances that I have gleaned from the writings of de Vries, Weismann and others, and those recently published in SCIENCE, but all of these rightfully belong to others; I have, however, cited some of them. Out of my own studies I can produce evidence in favor of the general theory of evolution, I can present phylogenies of genera and species that, I think, will stand the test of rigid criticism, I can furnish examples of the adaptation of structures, I can also show instances of variations in accordance with varying physical conditions, but I do not know a single fact relating to the Madreporaria that would aid in forming a definite conclusion regarding the origin of variation or the means by which adjustment is affected—I repeat, 'or the means by which adjustment is affected,' for the expression 'natural selection' is mostly used to raise a cloud of mental dust behind which we escape into our ignorance.

I should like to say that the controlling influences that govern the distribution of corals are being studied as assiduously as

possible by several men, and the subject has been given a certain amount of attention by nearly all recent students of zoophytes. We are obtaining more information on the physical determinants in the distribution of these organisms, but no one will be able to furnish more than an hypothetical explanation of the facts now accumulating until the conclusions are tested by experiments. Corals that grow in shallow water are fortunately easily experimented with, and I have hoped that the officials of the Carnegie Institution might undertake some work with them. Dr. C. Montague Cooke, of Honolulu, has told me that he intends undertaking a series of experiments on the reefs on the south coast of the Island of Molokai. Probably within a few years it will be possible to present definite data from the *Madrepora* on the questions now especially under consideration.

T. WAYLAND VAUGHAN.

SCIENTIFIC BOOKS.

Les tremblements de terre. Par F. DE MONTESSUS DE BALLORE. Paris, Libraire Armand Colin.

In Vol. IV., 1900, of *Beiträge für Geophysik,* Major de Montessus published a tabular statement of the seismicity of the various portions of the earth, divided into provinces. In the computation, 131,922 earthquakes were used and 10,499 epicenters; numbers far exceeding what had been compiled by preceding systematists taken all together. It was the work of many years, and from the mass of evidence distributively grouped he drew certain important conclusions. They were briefly as follows: (1) In a group of adjacent seismic regions, the most unstable (*i. e.,* most affected by quakes) are those which present the greatest differences of topographic relief. (2) The unstable regions are associated with the great lines of corrugation of the terrestrial crust. (3) Rapidly deepening littorals, especially if they border important mountain ranges, are unstable, while gently sloping littorals are stable, especially if they are the continuations of flat or slightly accidented coastal plains. (4) Though it is possible to indicate regions which present both volcanoes and earthquakes, there is no proof of interdependence between seismicity and volcanicity in general. While there are earthquakes which are certainly of volcanic origin, the one phenomenon does not necessarily imply the other. These views have been borne out and have been generally adopted by seismologists in the period of six years since they were promulgated.

But de Montessus seems to have been unwilling to let the matter rest. The inferences he drew in 1900, indeed, have not been abandoned. They, however, express the relations of seismicity to topography, and not to the causes of earthquakes, which were the real objects of his grand research. He has, therefore, taken up the subject anew, rearranged his facts, added to their number and made new generalizations from a geological as well as a topographical standpoint. And the new generalizations are of even greater interest and more striking than those of 1900. These are set forth briefly in the introductory chapter of the publication before us.

According to this analysis, earthquakes occur about equally, and almost exclusively, in two great circles or zones, which make with each other an angle of 67°. These zones are (1) the Mediterranean, or Alpine-Caucasian-Himalayan, which includes 52.57 per cent. of the quakes, and (2) the circum-Pacific Andean-Japanese-Malayan, which includes 38.51 per cent. of the quakes. These two zones coincide with the two most important lines of relief of the earth's surface. The poles of these great circles are situated 45° 45′ N., 150° 30′ W., and 35° 40′ N., 23° 10′ E., respectively.

This relation, which so far is purely geometric, calls for a geological interpretation, which may be read at once on the geological map of the world. The zones which include the seismic regions coincide exactly with the geosynclinals of the mesozoic age as they are figured by Haug in his well-known memoir, 'The Geosynclinals and the Continental

Areas.'[1] This, in general, is the synthetic law putting seisms into direct dependence upon the principal recent movements of the earth's crust, since it is along those zones that they have attained their greatest amplitudes, positive or negative. As a consequence of pure statistics and pure observation, without introducing any hypothesis, this law may be formulated as follows: "The geosynclinals, where the sediments deposited in the greatest mass have been energetically folded, dislocated and elevated in Tertiary time with the formation of the principal existing mountain chains (or geanticlinals), contain within themselves alone, with two or three doubtful exceptions, all the seismic regions, which consequently characterize them."

The geosynclinals more ancient than Mesozoic, which at various epochs have given place to plicated mountain chains, now eroded and hardly discernible in their present state of peneplains, present the peneseismic regions—the remains of ancient seismic regions which are now tending to stability. The continental areas (in the sense in which Haug uses the term), whose tabular architecture proves them to have always been the seat of collective movements of small amplitude and without large derangements of the subjacent strata, are very generally aseismic or barely peneseismic. In fact, one may say tersely, "The folded architecture of the geosynclinals is unstable, and the reverse is true of the continental areas, and the same has probably been true of all geological periods."

The body of the book is occupied with the discussion of the earthquakes of the different regions of the world, chiefly in their geological relations. It is a wonderful display of learning. To give any idea of it is entirely beyond the scope of this article. The only way is to buy the book and read it.

C. E. Dutton.

Electricity in Every-day Life. By Edwin J. Houston, Ph.D. 3 vols., 5½ by 8 inches, containing respectively 584, 566 and 609 pages. New York, P. F. Collier & Son.

This book has been prepared with the evident purpose of being sold to the lay public irrespective of its possession of scientific knowledge, of education, or of taste for books that improve the mind. It is, therefore, a good example of the modern art of book-making. It is attractively bound in cloth, much as would be a modern novel. It is illustrated by a few full-page plates in color, by a number of full-page half-tones in black and white and by a profusion of ordinary cuts. The subjects of the color plates are 'Edison in his laboratory,' 'Franklin and his kite,' 'aurora borealis,' 'a central station,' 'the broomstick train,' 'electricity on the stage,' 'Holz-machines in electro-therapeutics' and 'landing a sub-marine cable.' The black and white plates are of such subjects as electricity in the kitchen, the hat factory, the dairy, the tailor-shop, the mine and the composing-room. The other cuts will, many of them, be familiar to all those versed in the art, having many of them originated in S. P. Thompson's 'Elementary Lessons on Electricity and Magnetism,' in Ganot's 'Physics' or the catalogues of the makers of philosophical and scientific instruments. The first volume treats of the 'Generation of Electricity and Magnetism,' and the remaining two of the 'Electric Arts and Sciences.' The second volume treats of dynamos, electric lighting and electric power, and the third volume of electro-chemistry, telephony, telegraphy, annunciators and alarms, electric heating and electro-therapeutics. The style is popular, non-mathematical, clear, easy and attractive, considering the subject matter. Each chapter is introduced by a pertinent quotation from the classics or from the writings of men eminent in the profession. Marginal subject notes accompany the more important paragraphs and are of great service to the reader. Each volume has a very complete index. The early history of the various subjects treated is emphasized and because of the giving of dates, references and frequent extensive quotations should render the book of considerable service to those interested in patent litigation.

Samuel Sheldon.

[1] *Bull. Soc. Geol. France*, III., Series XXVIII., 633.

Mental and Moral Heredity in Royalty. By FREDERICK ADAMS WOODS, M.D. New York, Henry Holt. 1906. Pp. viii + 312.

Dr. Woods presents here in book form his investigations, reported originally in *The Popular Science Monthly,* of the family resemblances and of the comparative importance of original nature and environment as determinants of human achievement in the case of some six hundred related individuals. These were selected at random and graded by at least approximately objective criteria for intellect and also for morality. Dr. Woods states the sources of his information and the nature of his procedure clearly and in full detail, so that any one who doubts his conclusions can repeat the research.

These conclusions are presented in two ways, first by a series of descriptions of the facts of heredity in the leading stocks of European royalty and second by a more general account of the amount of resemblance found in related individuals and of the evidence which proves this resemblance to be due almost exclusively to the similarity in conditions of birth rather than of breeding.

The descriptions of royal houses make up fourteen chapters, all excellent in substance but necessarily somewhat unattractive and difficult to follow in parts unless the reader already knows the history of European courts well or has a pronounced taste for genealogy. One hundred and four most interesting portraits accompany these chapters. The comments on general issues, such as the supposed progressive degeneration of royal families, the 'Hapsburg lip' and prepotency, and the correlation of the eminence of ruler with the prosperity of the ruled in the case of Portugal, are both excellent in substance and interesting and relieve the monotony of individual descriptions. In the general summary given in the two closing chapters, the following estimates of resemblance are given:

	In Intellect.	In Morality.
Resemblances of offspring to father	.3007	.2983
Resemblances of offspring to grandfather	.1606	.175
Resemblances of offspring to great-grandfather	.1528	

The resemblance of husband to wife was found to be only .08 (with a possible error of .076) in the case of intellect.

The resemblance of intellect and morality within the same individual was .3403.

Dr. Woods gives as evidence that similarities in original nature are the cause of at least nine tenths of the resemblance in intellect and, perhaps, of an approximately equal proportion of the resemblance in morality the following facts: The resemblance to the maternal great-grandfather equals .1528; the likeness in morality to the maternal grandfather is as great as to the paternal grandfather; eldest sons who inherit the opportunities of a sovereign show no higher achievements than their brothers.

Dr. Woods is convinced that alternate inheritance is the rule in mental qualities and even uses this as an accepted fact in arguing that environment must be comparatively unimportant because children of the same nurture often differ markedly in intellect and morals. He fails, however, to submit the matter to any of the tests by which blended and alternate inheritance can be distinguished.

There are throughout certain matters of method and of interpretation in respect to which historians and psychologists will differ with Dr. Woods. The only ones of much importance are his ignoring of the fact that all his data on morality concern moral superiority or inferiority in comparison to the status of one period of civilization rather than absolute morality, his possibly hasty acceptance of alternate inheritance in mental and moral qualities and his failure to give in any convenient form the data which will permit any one to repeat the purely statistical portion of his work by possibly better methods. It is to be hoped with respect to the latter point that he will soon print somewhere tables of the individual relationships from which his mass results are calculated.

On the whole we must all admire the energy and persistence which enabled Dr. Wood to carry through so elaborate a study and the general sanity and impartiality of spirit with which he has made his inferences. In the reviewer's opinion precise objective measure-

ments of living men and women will be our final criterion of the strength of mental inheritance, but Dr. Woods's work is an important contribution to psychology and a most admirable lesson to show that history may become a natural science.

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SCIENTIFIC JOURNALS AND ARTICLES.

To the January-February *Journal of Geology*, the opening number of Volume XIV., Dr. S. W. Williston contributes a paper entitled 'American Amphicœlian Crocodiles' and Professor R. S. Tarr an article on 'Glacial Erosion in the Finger Lake Region of Central New York.' E. C. Andrews, of Sydney, New South Wales, discusses 'The Ice-Flood Hypothesis of the New Zealand Sound Basins' and S. W. McCallie describes some 'Stretched Pebbles from Ocoee Conglomerate.' This article is illustrated by three figures. Professor A. P. Coleman, in a paper entitled 'Pre-Cambrian Nomenclature,' reviews certain features of the report of the committee of American and Canadian geologists appointed by the surveys of the two countries to decide upon a suitable nomenclature for the pre-Cambrian formations of the Upper Lakes. The number concludes with an interesting editorial on the revival of the Illinois Geological Survey.

THE April number (volume 7, number 2) of the *Transactions of the American Mathematical Society* contains the following articles:

C. E. STROMQUIST: 'On geometries in which circles are the shortest lines.'

G. A. BLISS: 'A generalization of the notion of angle.'

OSWALD VEBLEN: 'The square root and relations of order.'

EDWARD KASNER: 'The problem of partial geodesic representation.'

R. P. STEPHENS: 'On the pentadeltoid.'

G. A. MILLER: 'The groups of order p_m which contain exactly p cyclic subgroups of order p^a.'

W. A. MANNING: 'Groups in which a large number of operators may correspond to their inverses.'

OSWALD VEBLEN and W. H. BUSSEY: 'Finite projective geometries.'

W. B. FORD: 'On the analytic extension of functions defined by double power series.'

L. E. DICKSON: 'On quadratic, hermitian and bilinear forms.'

PAUL STÄCKEL: 'Die kinematische Erzeugung von Minimalflächen.'

OSKAR BOLZA: 'A fifth necessary condition for a strong extremum of the integral $\int_{x_0}^{x_1} F(x, y, y')\,dx$.'

G. A. BLISS and MAX MASON: 'A problem in the calculus of variations in which the integrand is continuous.'

THE April number (volume 12, number 7) of the *Bulletin of the American Mathematical Society* contains: Report of the February Meeting of the Society, by F. N. Cole; Report of the Fifty-fifth Annual Meeting of the American Association for the Advancement of Science, by L. G. Weld; 'A Proof of the Fundamental Theorem of Analysis Situs,' by G. A. Bliss; 'Determination of Associated Surfaces,' by Burke Smith; 'Note on the Practical Application of Sturm's Theorem,' by J. E. Wright; 'The Movement for Reform in the Teaching of Mathematics in Prussia,' by J. W. A. Young; Review of Jahnke's Vorlesungen über die Vektorenrechnung, by E. B. Wilson; Review of Moulton's Introduction to Celestial Mechanics, by A. O. Leuschner; Shorter Notices: Borel's Géométrie, Premier et Second Cycle, by C. L. E. Moore; Schüssler's Orthogonale Axonometrie, by Virgil Snyder; 'Notes' and 'New Publications.'

The May number of the *Bulletin* contains: Report of the February Meeting of the San Francisco Section, by G. A. Miller; 'An Application of the Theory of Differential Invariants to Triply Orthogonal Systems of Surfaces,' by J. E. Wright; 'Surfaces generated by Conics cutting a twisted Quartic Curve and an Axis in the Plane of the Conic,' by Virgil Snyder; 'Operation Groups of Order $p_1^{m_1\mu_1} p_2^{m_2\mu_2}$,' by O. E. Glenn; 'A Definition of Quaternions by Independent Postulates,' by Miss R. L. Carstens; 'Note on the Heine-Borel Theorem,' by N. J. Lennes; Review of Borel's Leçons sur les Fonctions de Variables Réelles, by J. W. A. Young; Shorter Notices: Hawkes's Advanced Algebra, by G. D. Olds, Brioschi's Works, by H. S. White,

Verhandlungen des dritten Mathematiker-Congresses, by H. S. White; 'Notes' and 'New Publications.'

The Botanical Gazette for April contains the second paper of Dr. E. W. Olive on 'Cytological Studies on the Entomophthoreae,' being a presentation of the nuclear and cell division of Empusa; a second paper by Professor V. M. Spalding on the 'Biological Relations of Desert Shrubs,' in which the results of experimental work on the absorption of water by leaves is presented; descriptions of numerous new species of Californian plants, by Miss Alice Eastwood; and a sixth paper on North American grasses, by A. S. Hitchcock. The usual book reviews and notes for students close the number.

SOCIETIES AND ACADEMIES.

THE GEOLOGICAL SOCIETY OF WASHINGTON.

AT the 175th meeting of the society on February 14, the following papers were presented:

Paleozoic Stratigraphy of China: BAILEY WILLIS.

Mr. Willis presented certain stratigraphic results of the Carnegie expedition to China of 1903–4 for geological research. Stratigraphic sections were carefully observed in the northeastern province, Shan-tung, in northern Shan-si, and in the central region of south Shen-si and Ssi-ch'uan. In general, the Paleozoic system is extensively represented from late lower Cambrian to Carboniferous or Permian. The basement on which it rests is commonly a metamorphic complex of the general character of the Archean, but locally at least two pre-Cambrian systems, one of which resembles the Huronian and the other the Belt Mountain series, are distinguishable. In north China, north of latitude 34°, the Cambrian and Ordovician constitutes a continuous sequence of limestones, with occasional interbedded shales, about 3,500 feet thick. The basal shale, 350 to 500 feet thick, is distinguished by the prevailing red color of the sediments. The unconformity at the bottom is one of marine plantation across a previously developed peneplain, and is very even. There is no lithologic break at the top of the Cambrian, the passage to the Ordovician occurring in the body of limestones and being recognized only by the fossils. At the top of the Ordovician there is an eroded surface which closely parallels the bedding of the limestones, but exhibits hollows which are occasionally ten or fifteen feet deep and are filled with clay that is useful for pottery. Upon this surface rest shales which carry upper Carboniferous (Pennsylvanian) fossils and contain coal beds. In the upper part of the coal-bearing measures basaltic flows are interstratified with shales. Cross-stratified red sandstones succeed, and above these come in sandstones with coals which carry Jurassic plants. The sequence resembles that of the Permo-Mesozoic of India.

In central China the Paleozoic sequence differs from that in north China in several respects. At the base on the Yang-tzi there is a granite which may be Archean or Algonkian. It is locally overlain by 150 feet of quartzite, upon which rests an early Cambrian glacial till. Limestones approximately 4,500 feet thick rest upon the till and include in the lower layers a conglomerate containing pebbles derived from the till. These limestones represent the Cambro-Ordovician sequence and carry at their top a rich fauna of Trenton age. They pass by transition into thin bedded shales which are in part Carboniferous, suggesting the Devonian black shale of the Appalachian region, and in part greenish shaly sandstone like the Chemung. This formation is about 1,800 feet thick and represents all the Middle Paleozoic. It is overlain in apparent conformity by a limestone 4,000 feet thick, which contains coal beds and about 1,000 feet above its base yielded upper Carboniferous fossils of late Pennsylvanian association. From a layer at the contact of the limestone with the shales a few obscure forms, which may be late Devonian or lower Carboniferous, were obtained. Above the Carboniferous limestone comes in the sequence of Red Beds with thin marine limestone and coal beds, which some 600 or 800 feet above their base contain Jurassic plants. Thus in central

China we seem to have a continuous sequence of Paleozoic strata with very meager development of the Silurian and Devonian, and with a great limestone in place of the Coal Measures of Shan-si and Shan-tung.

Natural Coke in the Wasatch Plateau: J. A. Taff.

Mr. Taff described the occurrence of natural coke from two localities in the Book Cliffs coal field near the north end of the Wasatch Plateau in central Utah. The coal, of which there are extensive deposits, crops out in the Book Cliffs and eastern escarpment of the Wasatch Plateau. Along the outcrops the coals have been extensively burned, presumably by spontaneous combustion fusing the overlying strata in places into vesicular slag-like masses. At one of these localities, about ten miles southwest of Castlegate, an outcrop of metamorphosed coal having the columnar structure and luster of coke was found in association with fused siliceous deposits.

The second locality is at the Winterquarters mines, two miles west of Scofield. Dikes of an igneous rock ten feet in width have cut vertically across the coal bed, nine to sixteen feet thick, metamorphosing the coal into a coke-like substance to a distance of three feet on each side. The coal thus fused is distinctly columnar, the columns standing perpendicular to the face of the dike, and has a graphitic luster, but is not vesicular like artificial coke. While it has the structure in part and the luster of artificial coke it has not the composition. The following are analyses of the coal and coke from the same mine, made by the U. S. Geological Survey.

	Coke, Per Cent.	Coal, Per Cent.
Water	0.32	8.10
Volatile hydrocarbon	20.38	40.20
Fixed carbon	65.90	45.91
Ash	13.10	5.76

The hypotheses presented in explaining the composition are: (1) that the coal at the time of the metamorphism was deeply covered and excluded from the atmosphere, thereby preventing the escape of all the volatile hydrocarbons; (2) that the metamorphosed coal after having lost a large part of its volatile matter has become enriched to a certain extent by the gaseous products emanating from the surrounding coal; (3) that the natural coke, a product of the metamorphism of coal, is arrested by the cooling of the igneous mass before sufficient time was given for the escape of all the volatile hydrocarbons.

The Glaciation of certain Quartzite Ledges of Southeastern Wisconsin, and Boulder Trains derived therefrom: W. C. Alden.

This investigation will be published by the Geological Survey.

The meeting of the society on February 28, 1906, was devoted to a general discussion entitled 'The Early Paleozoic Succession in the Appalachians and the Effect of Barriers on the Distribution of the Formations and Faunas.' The members contributing to the discussion were Messrs. Bassler, Keith, Ulrich and Willis. The interest of fully twenty-five members was aroused to such a degree that three special conferences have been held for a continuation of the discussion.

On March 14, the following program was presented:

Maximum Glaciation in the Sierra Nevada (illustrated): Mr. Willard D. Johnson.

Geological Reconnaissance of the Coast of Olympic Peninsula (illustrated): Mr. Ralph Arnold.

On March 28, under the head of 'Informal Communications,' Dr. David T. Day described a sand storm observed by him in the Columbia River Valley in Oregon. In this district, though the ordinary so-called Chinook winds are continually moving the sands brought down by the river, the Oregon Railway and Navigation Company has been able to protect its tracks by placing boards inclined at an angle of 45° to the line of the tracks at critical points. The storm described was caused by a violent wind blowing in a direction opposite to that of the prevailing winds. The amount of sand moved was very large, a rough measure being furnished by the observation of four feet of sand accumulated in box cars standing with one door open toward the storm. From sam-

ples collected it was found that the greater part of the sand passed a one-hundred-mesh sieve, though sixteen per cent. of the material was coarser than twenty-mesh. In the latter portion, however, it is to be noted that there is a large amount of cinder, doubtless derived from locomotives.

Dr. Geo. P. Merrill exhibited one of fourteen fragments collected by various parties from a meteorite of the peridotite type which fell near Fort Scott, Kansas, September 2, 1905. Attention was called to the fact that the exterior of all the pieces of this meteorite which was seen and heard to explode are fused.

Regular Program. Report of Mr. S. F. Emmons as U. S. Delegate to the Congress of Applied Geology held at Liége, Belgium, June, 1905.

This was the first international congress devoted exclusively to economic geology, and its sessions were held simultaneously in four independent sections: mines, metallurgy, mechanics and applied geology, only the last of which the delegate was able to attend.

Liége is an appropriate place for the meeting of such a congress, being a center of old and well-established coal, iron and zinc industries, the seat of an important school of mines, and, during the past summer, the scene of an international exposition commemorative of the seventy-fifth anniversary of the independence of Belgium.

Mr. Emmons presented a summary of the geological and industrial conditions of the valley of the Meuse in which the flourishing and picturesque city of Liége is situated, and gave an abstract of the more important papers discussed by the congress under the following heads: (1) tectonics, (2) coal and petroleum, (3) ore deposits, (4) hydrology.

The first discussion had mainly to do with the structure of the Belgian coal basin in which the Paleozoic rocks are compressed into a series of anticlines and synclines, and complicated by many overthrust faults. It also considered the probable extension of the coal-basin rocks of Alsace-Lorraine beyond the boundary into France in the region where they are buried by the transgression of Mesozoic beds to depths of 2,000 to 2,500 feet. It is, of course, a question of great industrial importance to France whether the workable coal beds may be developed within their boundaries in that region.

The question as to the origin of coal and petroleum was discussed in a most interesting manner by Professors Bertrand, of Lille, and Potonié, of Berlin, based on studies with the microscope and in the field; a brief statement was given of the latter's theories, which are entirely on the autochthonous side of this much-mooted question.

In metallic ore deposits the discussion touched the genesis of the zinc ores of Belgium and ores of mercury and other metals in Italy, besides making a brief mention of platinum-bearing placers in the Congo.

The discussion on hydrology involved the fresh-water-bearing dunes along the shores of the North and Baltic seas, from which in Holland the city of Amsterdam draws its supply of potable water.

Interesting considerations were also presented by French hydrologists on the influence of forests and deforestation in different European countries on their resources of white fuel or water (*la houille blanche*).

The Hamilton Mine, New Mexico: W. LINDGREN.

The Santa Fe Range, in northern New Mexico, consists chiefly of pre-Cambrian granites, gneisses and schists. Among the latter are many smaller masses of amphibolites, evidently derived from pre-Cambrian intrusions of basic igneous rocks in the granite. Carboniferous beds, chiefly sandstones and limestones, occupy large areas on the east side of the range on both sides of the upper Pecos River, and are separated from the pre-Cambrian crystalline rocks by a fault of great throw. The Hamilton mine is situated on the upper Pecos River about twenty-five miles east of Santa Fe, at the place where the erosion by the river has exposed a small part of the pre-Cambrian basement underneath the Carboniferous beds. The latter contain small but workable coal mines, and the fossils indicate a Pennsylvanian age. The amphibolites,

which strike in a northeasterly direction, contain a deposit of chalcopyrite, zinc blende and pyrite which has been worked intermittently since 1885. The workings consist of a shaft 250 feet deep, together with drifts and crosscuts extending for a short distance along the deposit. The ore minerals are directly imbedded in the amphibolite and the gangue minerals consist of biotite, amphibole, tourmaline and quartz. Interest centers in the age of the deposit, which is clearly and unmistakably older than the Carboniferous beds which cover the decomposed and oxidized croppings of the ores. Unquestionably it is also pre-Cambrian, for Cambrian strata are present in both southern Colorado and southern New Mexico. Their absence in this particular region indicates simply that this vicinity constituted a land area during the earliest Paleozoic times.

Examination of other points in the ranges north of Santa Fe by the author and Mr. L. C. Graton, revealed the fact that at a great many other points pre-Cambrian deposits also exist, although in few cases was it possible to prove it so conclusively as was the case at the Hamilton mine. Details of these occurrences will be described in a forthcoming reconnaissance report of the mineral deposits of New Mexico. In general, there are two types of deposits, both of which most frequently contain chalcopyrite. The first consists of stringers and irregular lenses of quartz and calcite enclosing copper ores; the second forms disseminated ores or 'fahlbands' in schists, either amphibolites or gneisses.

It has been known for a long time that the gold deposits in the southern Appalachian states are of early Paleozoic or pre-Cambrian age. The pre-Cambrian age of the Homestake mine in South Dakota has also been firmly established. The author indicated some years ago the probability of pre-Cambrian deposits occurring in Wyoming, and recently Dr. A. C. Spencer has shown that the copper deposits of Grand Encampment, in the same state, most likely also belong to the same early period. This leads to the inference that pre-Cambrian deposits must be present in Colorado.

The pre-Cambrian deposits, as stated above, are apt to contain copper as the most valuable metal. Their tenor in gold and silver is usually low.

Arthur C. Spencer,
Secretary.

THE TORREY BOTANICAL CLUB.

The meeting of November 29, 1905, was held at the New York Botanical Garden, with Vice-president Underwood in the chair. Twenty persons were present.

Dr. D. S. Martin exhibited specimens of glassy cinders formed by the burning of masses of rice-hulls near Charleston, S. C., illustrating in a striking manner the presence of silica in these hulls.

The announced paper of the afternoon was by Dr. N. L. Britton, under the title of 'The North American Cactaceæ.' The speaker remarked that the Cactaceæ of North America were being carefully studied by himself in cooperation with Dr. J. N. Rose, of Washington, in anticipation of preparing a systematic account of this group for the 'North American Flora.' The Mexican forms have been extensively collected by Dr. Rose and are being kept under cultivation in Washington and New York. Numerous species from Arizona, New Mexico, Lower California and the West Indies have been secured by expeditions sent out by the New York Botanical Garden and now are under cultivation in New York. Herbarium material is, as a rule, peculiarly inadequate to a proper appreciation of the relationships of the members of this family and it is hoped soon to have all of the North American species under observation in the living state. Herbarium specimens are being supplemented by photographs and by material preserved in fluids.

The most recent of the more important papers on the classification of the Cactaceæ is one by Berger, entitled 'A Systematic Revision of the Genus *Cereus* Mill.' and published in the Sixteenth Report of the Missouri Botanical Garden (1905). This paper has been based chiefly on the studies made in

Sir Thomas Hanbury's famous gardens in Italy, and gives much importance to characters of flowers and fruit, characters which have been largely ignored in previous schemes of classification because unknown. The genus *Cereus* is divided into eighteen subgenera by Berger. The studies of the speaker and of Dr. Rose indicate that both in the old genus *Cereus* and in other groups of the cactus family, well-marked differential characters of flower and fruit are coordinated with those of the stem in such a way as to make the recognition of several new genera natural and convenient. After these introductory remarks, the meeting was adjourned to the propagating houses of the garden, where numerous living specimens of Cactaceæ were demonstrated and commented upon. Of the genus *Cereus* in the current sense, various types representing subgenera or possible generic segregates were discussed. Among these were *Cereus peruvianus,* the proper type of the genus *Cereus;* species of the *Pilocereus* group, with which the older *Cephalocereus* is historically identical; *Cereus* Schottii of Berger's subgenus *Lophocereus; Cereus geometrizans,* representing Console's genus *Myrtillocactus; Cereus Pringlei* of Berger's group *Pachycereus; Cereus sonorensis,* representing *Stenocereus,* also of Berger; *Cereus triangularis,* a species much cultivated in the West Indies and southern Florida, with large beautiful nocturnal flowers, a member of Berger's subgenus *Hylocereus; Cereus grandiflorus,* the best-known night bloomer, belonging in Berger's subsection *Selenicereus;* the curious *Cereus Greggii* with slender stem and very large tuberous subterranean part, representing the subsection *Peniocereus* of Berger; the Central American *Cereus baxaniensis* of the group *Acanthocereus;* the Costa Rican *Cereus Gonzalezii,* of Berger's subgenus *Leptocereus;* and also representatives of Engelmann's subgenus *Echinocereus.* Other specimens were exhibited to illustrate the genera *Phyllocactus, Epiphyllum, Cactus, Echinocactus, Melocactus, Ariocarpus, Pelecyphora, Rhipsalis, Opuntia, Nopalea,* and the curious *Pereskia,* with its leafy, vine-like or shrubby stems.

THE meeting of December 12, 1905, was held at the American Museum of Natural History, with President Rusby in the chair. Thirty-four persons were present.

The announced paper of the evening was by Dr. Henry Kraemer and was entitled 'Some Studies on Color in Plants and the Artificial Coloring of Flowers.' The subject of color in plants was considered first from a morphological and chemical point of view, and the speaker performed various illustrative chemical experiments involving changes of color in liquid media. The results of numerous experiments on the control of color in living plants and on the artificial coloring of cut flowers were given. Dr. Kraemer's paper will be published in full in the *Bulletin* of the club. The following is his abstract of the more important results of his observations and experiments:

1. Unorganized or cell-sap color substances are distributed usually in largest amount at the termini of the branches, as in flowers and terminal leaves, or in roots, or in both tops and roots. Their occurrence in those portions of the plant, which are young and growing, points to the conclusion that they are not to be disregarded in the study of metabolic processes. Goebel holds a similar view. He says that it is 'very probable that the feature of color which so often appears when the propagative organs are being brought forth has some connection with definite metabolic processes, although till now we can not recognize what these are.'

2. The distribution of the so-called flower color substances in other parts of the plant than the flower also points to the same conclusion, and that the part which they play in attracting insects to flowers, is, if indeed they have any function of this kind, incidental rather than fundamental. The fact that certain colored flowers, as in the spruce and red maple of early spring, are pollinated by the wind, would tend to confirm this view. The food in the nectar and pollen are no doubt sufficient attraction for insects and other animals.

3. The occurrence of chromoplastids in a reserve organ, as in the tuberous root of the carrot, and the similar occurrence of chromoplastids and of reserve starch in the petals of the buttercup, lead to the inference that the petal of the buttercup, like the root of the carrot, has the function of storing nutrient material. In each case cells containing chromoplasts rich in nitrogenous sub-

stances are associated with cells containing reserve materials. In the case of the carrot the reserve materials are utilized by the plant of the second year, and in the case of the buttercup they are utilized in the development of the akene.

4. The feeding of plants with chemicals for the purpose of controlling color, as certain iron, aluminum, potassium and other salts as well as certain organic acids, has not so far, in the author's experiments with carnations, roses and violets, produced any marked changes in the colors of the flowers, only some slight effects being noted which might be attributed to other causes. Knowing that plants have a certain individuality and certain inherent qualities or tendencies, other than negative results could hardly be expected. On the other hand, the plant is a rather plastic organism, and for this reason experiments along the line indicated are more or less justified.

5. Experiments in supplying plants and cut flowers with vegetable coloring matters and aniline dyes showed that none of the vegetable color substances were taken up and that only a comparatively few of the aniline dyes would color flowers. The fact that of thousands of dyes or color substances, only a few are carried as high as the flower, would tend to show that only certain chemicals or substances would be taken up by the plant, and thus exert an influence on the coloring matter in the flower. If such profound changes occur in plants as are provided by the mutation theory, is it too much to suppose that certain definite changes might be produced by means of which we have knowledge or control?

Dr. Kraemer's remarks were illustrated by a hundred or more freshly cut flowers such as carnations, roses, hyacinths and callas, which had been artificially colored in the few hours preceding the demonstration by placing the stalks of the flowers in solutions of certain dyes. Numerous dried specimens of artificially colored flowers of various plants were also exhibited.

Dr. Rusby showed fresh fruits of the so-called 'tree-tomato,' a species of *Cyphomandra* native to South America.

Marshall A. Howe,
Secretary pro tem.

THE ST. LOUIS CHEMICAL SOCIETY.

The regular meeting of the society on March 12 was devoted to a consideration of matters connected with the organization of the society.

April 9. The paper of the evening, communicated by the secretary, was entitled 'Phosphorescent Zinc Sulphide.' The paper dealt with phosphorescent zinc sulphide prepared by Mr. John Esmaker. The method followed was detailed briefly, with a view to emphasizing the slight changes in procedure, which result in failure to obtain the phosphorescent variety of zinc sulphide. The changes in the method were so slight, that apparently they should have had no influence on the character of the result. The remainder of the paper, and the general discussion which followed, dealt with phosphorescence and similar phenomena, and endeavored to assign some reason for the observed effects.

C. J. Borgmeyer,
Corresponding Secretary.

DISCUSSION AND CORRESPONDENCE.

A PLAN TO ENSURE THE DESIGNATION OF GENERIC TYPES. AN OPEN LETTER TO SYSTEMATIC ZOOLOGISTS.

Probably no other single factor has caused so much confusion in systematic zoology and nomenclature as has the failure on the part of some authors to definitely designate the type species for the new genera they have described. Such failure, indeed, so frequently produces confusion, that the suggestion has been made that a rule be inserted in the International Code of Nomenclature to the effect that no new generic name proposed after a given date, say December 31, 1909, may claim recognition unless its author definitely designates its type at the time of the publication of the name in question. A rule of this nature, extreme though it may appear to some persons, seems to be fully warranted in view of the experience zoologists have had with genera proposed without types. It seems somewhat doubtful, however, whether the International Congress would see its way clear to adopt the proposition just referred to.

Another plan has occurred to me by which practically the same result may be obtained, without recourse to the adoption of the pro-

posal mentioned, namely, by inducing journals and publishing societies to refuse publication to papers containing new genera for which the authors fail to designate types. This plan, unbeknown to me at the time, had already been adopted by the Washington Biological Society before I began to advance it. I have now brought the proposition before several organizations, all of which have agreed to insist upon the designation of a type for every new generic name submitted to them for publication, and instructions have been issued to the general effect that papers not complying with the rule will not be accepted for publication. The organizations which have notified me of the adoption of this general plan are as follows:

U. S. Fish Commission.
U. S. Geological Survey.
U. S. Department of Agriculture.
U. S. National Museum.
U. S. Public Health and Marine Hospital Service.
Smithsonian Institution.
Biological Society of Washington.
Entomological Society of Washington.
American Museum of Natural History, New York.

It is my intention to communicate with other organizations in the hope of inducing them to adopt this same plan. Such a movement, however, when dependent upon the efforts of one person, is necessarily somewhat slow. On this account I take the liberty of addressing the systematic zoologists, through SCIENCE, and of asking them to join in the movement by bringing the matter before any publishing organizations to which they belong and by urging its adoption not only by societies, academies, surveys, etc., but also by zoological journals.

I shall be under obligations if zoologists will notify me of any societies, journals, etc., which have already adopted this rule, or which adopt it in the future.

CH. WARDELL STILES.

CERTAIN PLANT 'SPECIES' IN THEIR RELATION TO THE MUTATION THEORY.

AT the last congress of the American Ornithologists' Union I presented a short paper on the 'Applicability of the Mutation Theory to Birds.' My conclusions were entirely in accord with those of Dr. C. Hart Merriam as presented in his most interesting address before the American Association in New Orleans.[1]

There is one point, however, not touched upon by Dr. Merriam which I brought forward as probably influencing de Vries or at least others who share his views. This is that we seem to have among plants certain forms which are, so far as their differential characters are concerned, comparable to subspecies among terrestrial vertebrates, but which are not restricted to any definite geographic life area or climatic zone, as is always the case with the latter.

Any one at all in touch with modern botany is aware of the tremendous number of forms which are now being described as species. In order to learn something of the nature of these forms and their possible correlation with subspecies of birds and mammals, I selected the acaulescent violets and spent several years studying their variations in the neighborhood of Philadelphia.[2]

I found it possible to recognize a number of quite distinct forms, and yet every year I discover others of intermediate character; while every new section of country yields allied forms which do not fit exactly into any of my previously prepared diagnoses; yet each of these forms is reasonably constant in its own patch or neighborhood.[3] These are certainly not *species,* neither are they subspecies as we understand them in vertebrates. Moreover, it is hopeless to begin to 'lump' them, for we soon find ourselves forced to combine species of long standing and ultimately to have only one species of acaulescent blue violet[4] and one white one!

Just what these 'forms' are and what their

[1] SCIENCE, XXIII., p. 241.

[2] Cf. Stone, *Proc. Acad. Nat. Sci.*, 1903, p. 656.

[3] Cf. Burgess, 'Biotian Asters,' *Mem. Torrey Bot. Club,* XIII. Also Brainard, *Rhodora,* VI., 213; VIII., p. 6 and 49, where hybridism on a large scale is advanced as the explanation of these forms.

[4] Exclusive of *V. pedata* of course.

origin may be I am not prepared to say, but they have no counterpart among birds and with a few possible exceptions none among other vertebrates.

I may say that I do not regard them as the result of mutation as the followers of de Vries apparently do, but think it likely that they may be due to the action of immediate local environment, the exact nature of which it is practically impossible for us to detect. The extremely sedentary nature of plants, especially some groups, and the ease with which isolation may affect them would tend to emphasize the effect of local environment in producing differentiation.

In terrestrial vertebrates we find among snakes certain forms with peculiar coloration occurring as colonies here and there within the range of the species which do not conform to any definite geographic habitat, and in some fossorial or semifossorial mammals similar extremely local forms occur, as '*Geomys colonus*' Bangs surrounded by the range of *G. floridanus* and *tuza* and in just the same sort of environment so far as we can see; also '*Microtus rufidorsum*' Baird, which occurs in colonies within the range of *M. pennsylvanicus*.

These may be parallel cases to those exhibited in *Viola, Cratægus, Aster, Panicum,* etc., and their sedentary nature seems to point similarly to elements in the immediate local environment as the probable cause of their differentiation.

Witmer Stone.

Acad. Nat. Sciences, Philadelphia.

ISOLATION BY CHOICE.

To the Editor of Science: The recent discussion of isolation in Science reminds me of a popular article I wrote for *The Outlook*, emphasizing the psychic factor in evolution[1]—the part that choice plays. We must, it seems to me, not forget the various factors that work together at the same time in producing species. A fish with weak eyes would naturally prefer cave life, and thus isolated breed with others similarly equipped, physically and mentally. Those that have the physical variation without the mental would soon feel the effect of their want of sense.

The same principle applies in protective coloration. One may easily conceive of two habitats where the protective coloration would be quite different, and it is easy to see that the survival of those sensible enough to stick to the habitat best suited to them might quickly lead to intensification both of the tendency to seek the one habitat and of the coloration that adapts them to it. Indeed sexual selection may come in. Those having a willingness to accept mates with an erratic tendency to the other habitat or less protective coloration would have progeny less liable to prosper. Thus we may easily imagine two color races, species, arising, separated by a hereditary preference for different habitats, and for mates with all the peculiarities that those habitats have produced, while yet there is no physical barrier preventing the crossing, which may indeed go on to some slight extent.

Alfred C. Lane.

[1] February 18, 1898.

LARVAL CONGER EELS ON THE LONG ISLAND COAST.

The occurrence of larval conger eels in great abundance on the Atlantic coast has, as far as I am aware, not been recorded; accordingly the following note may be of interest.

On May 27, 1905, the 'Leptocephalus' of a conger eel appeared in great numbers at Easthampton, on the south shore of Long Island, about twenty miles from Montauk Point. They were washed up by the waves, literally, in thousands, and continued to come ashore in greater or less quantity—being especially abundant again on June 3—for about a fortnight. It was evident that this interesting harvest was due in some measure, at least, to a local storm and change of currents, which also brought in a number of bottom forms—*e. g., Natica* and its eggs.

The larvæ were all of a uniform length of about four inches, and in a few cases appeared to be in normal condition; most, however, were found to be either dead or dying. Dr. Bashford Dean, who has seen my specimens, tells me that they are probably *Leptocephalus*

conger. A larva of the same species sent to Dr. Dean by Professor Grassi, who collected it at Messina, is five inches in length.

L. S. QUACKENBUSH.

SHOULD OUR COLLEGES ESTABLISH SUMMER SCHOOLS?

A NEW and important feature in the educational scheme of our colleges is the growing tendency toward the establishment of summer schools. According to the report of the Commissioner of Education there were in 1903 over 11,000 students in the summer schools of 51 of our colleges. About two thirds of these students were women; mainly teachers of secondary schools. The number of students now attending the summer schools is about one tenth as great as the total number enrolled in our colleges throughout the year, and is more than twice as great as the number studying in the graduate schools of our universities.

The growth of these summer schools in America dates from 1874 when the religious assembly at Lake Chautauqua began the summer training of Sunday-school teachers, and in 1878 this movement grew into the establishment of a general summer school, aiming to disseminate culture chiefly among those who had not enjoyed the benefit of college training. In this the Chautauqua school has achieved well-renowned success, raised the general level of intelligent appreciation, and broadened the mental horizons of thousands; thus exercising a beneficent influence upon our national life not to be overestimated.

Such a summer school as that of Chautauqua, independent of any one college but dependent in a large sense upon all, aims chiefly to broaden rather than to deepen culture, and to maintain and develop the best standards of life and thought. It teaches, above all, that lives of the highest value to civilization may be devoted to the true and the beautiful rather than to the material side of progress. Its aim differs from that of the colleges in that it is extensive rather than intensive, broad rather than precise; developing thus a higher standard of general culture, rather than training specialists for professional careers.

Of late years, however, the summer school has become an established feature of the curriculum of our colleges themselves. These summer schools of the colleges are naturally centers of culture which as such must accomplish much general good, but they often hold out a false hope to those who visit them desiring to gain precise technical knowledge. One can not accomplish in six weeks what should be done in a year of patient study accompanied by laboratory experience. The tendency of these college summer schools is to substitute superficiality for depth, and to increase rather than diminish the number of half-trained specialists with which our country is already over-burdened.

But apart from their more or less beneficial effect upon the student I wish to call attention to an evil influence they are beginning to exert upon those who teach in our colleges.

Until within a few years the college teacher looked upon his summer vacation as a season for research and broadening study; now fully one half of this once cherished period must be sacrificed to the labor of the dissemination of superficial and elementary instruction.

What can we hope from our universities if the spirit of research, which already languishes, be killed within them. The intellectual achievement of our highest schools can be measured only by the standard of productive scholarship; not by the amount but by the quality of their instruction.

Men are not machines to be loaded with knowledge at one brief period in their youth, and then to impart wisdom unchanged throughout the remainder of their days, and yet this development of the summer school in connection with the college is surely cutting down those precious hours when the teacher himself becomes a student. What more stimulating to the teacher or beneficial to the college than a vacation rightly used in research, intelligent travel or in contact with his fellow men beyond the college walls.

Correlated with the growth of the summer school system is the tendency of the college itself to maintain low salaries for its instructors, relying upon the fact that by teaching in

the summer school the young instructor may earn sufficient for his maintenance.

In nearly all of our colleges the salary paid to the teacher below the grade of professor is so small as to render it all but necessary for him to devote at least a portion of the summer vacation to primary teaching. This evil chiefly affects the teacher who is neither old nor young, but who in the fullest possession of his newly developed abilities rejoices still in the full energy of youth, and yet is upon the threshold of that fuller knowledge which years alone can bring. Now if ever is he fitted for giving new thought to the world, and now of all times are his free moments precious.

In my official connection with a laboratory whose purpose it is to afford unrivaled facilities for research to those best fitted to avail themselves of the opportunity, I find that fully one third of our ablest investigators feel obliged to decline invitations to pursue research work free of all expense; and answer that in order to provide adequate support for wife and family they must forego the attractive prospect, and teach in the summer schools. And thus they must decline facilities for the solution of problems which years of training have best fitted them to solve, and to the solution of which their thoughts must turn with hope and longing.

Granted that research must generally be performed at a sacrifice to him who loves it, and that the genius of advancing thought flourishes best in adversity, is this an argument for rendering research well-nigh impossible, and for substituting the low achievement of expounding well-worn facts for the glory of discovery?

Our colossal universities are weak in research when compared with those of Germany, and when we look upon the great names of those who were among us we see that productive scholarship has not advanced with our material progress.

We must have more of the spirit of Agassiz who knew of the hidden wealth by Lake Superior's shore, but had not time to make money; of Henry who knew of the practical value of his electro-magnet but swerved not from his path and ever studied science simply for his love of it, never asking of it the rewards of wealth.

Not the least of this evil of which we speak is the fact that our colleges are putting into the high place the ideal of mere money getting. The most inadequate measure of success is thus lauded as the highest, and it is a lamentable fact that a large number of our leading college professors have deserted research to enter upon commercial careers.

Measure our universities by standards truly high. What character do they develop in their graduates, what love for research do they inspire, how thorough is their scholarship. Even small colleges may excel the great universities in these things. This matter has been most ably discussed by Professor Münsterberg in his book upon 'American Traits,' giving as he does the deferential but nevertheless just opinion of one who as a visitor among us contrasts our achievements in higher education with those of his native land.

It is to be hoped that one among our graduate schools may develop as an autonomous institution, with its own special faculty devoted exclusively to the advancement of its aims, and substituting the standard of original work for that of mere erudition, and of quality for that of quantity. If one among them thus should raise its head, all others soon would follow.

Alfred Goldsborough Mayer.
Marine Laboratory of the Carnegie Institution, Tortugas, Florida.

On the Origin of the Small Mounds of the Lower Mississippi Valley and Texas.

To the Editor of Science: Apropos of a communication under the above title by P. J. Farnsworth in your issue of April 13, 1906, I beg the privilege of a few remarks.

From time to time a number of notes have been written upon this subject in Science by Messrs. Veatch, Branner, Bushnell and perhaps others, but unfortunately none of these papers are available to me except those of Messrs. Farnsworth and Bushnell.

The mounds to which I allude are low,

circular eminences, averaging, say, twenty feet in diameter, seldom exceeding two feet in height, occurring thickly studded over extensive areas of both forest and prairie lands.

For nearly twenty years I have been observing these mounds in the second bottoms of the rivers of the southern coastal plain, the coast prairies of Louisiana and Texas, in southern Arkansas, Indian Territory and in the extension of the old valleys of the rivers toward the great plains.

I do not believe that they owe their origin to the uprooting of trees, the handiwork of man, to glacial agencies, or the pressure of underground gases, as some of your correspondents and others have alleged.

As to Mr. Farnsworth's theory that they are the result of uprooting trees: while I have seen many mounds in the forests which have thus been made, this hypothesis can not apply as a general explanation, owing to the fact that millions of the mounds occur on the newly made coast prairie of the Texas region which is not and has never been inhabited by forest growth. Two weeks ago I drove through thousands of these mounds on the mainland of Texas opposite Galveston Island, and any one familiar with the conditions of that portion of the coast prairie will immediately abandon the uprooting tree theory.

Mr. Bushnell's theory that these mounds were made by man is also totally inadequate. I am well aware of the fact that in poorly drained flat areas of the old flood plains and second bottoms of the Mississippi Valley and its tributary laterals, mounds do exist which were constructed by aboriginal hands, but the mounds under discussion are not of this class. I have seen hundreds of these dissected by roadways and other cuttings and they show no trace of human work. Furthermore, they are so numerous and extensive that their construction by men would have required a larger population than has ever yet inhabited these regions, or than it could possibly support.

The glacial theory can also be dismissed with the statement that in most instances these mounds inhabit non-glacial formations.

Another theory, which has probably been mentioned by some of your correspondents, is that the mounds are produced by the ascending gas above oil pools, has wide local adherence and should be discountenanced because of its economic misapplication. In fact, the name 'gas mound' now being used locally by the people for this class of phenomena in Texas is the only specific one which I have heard applied. Many charlatans and even misguided honorable men are holding to the 'gas-mound' theory and express astonishment when any one disputes it, notwithstanding the fact that the identical mounds are found to occur in many districts where not the least sign of oil or gas has been discovered. This theory has cost useless expenditure of many thousands of dollars in drilling for oil pools.

While frequently feeling, like Professor Forshey, as quoted in Mr. Farnsworth's article, that the more familiarity I have with these mounds, the less explicable they seem to me, I am of the decided opinion that they are natural products of certain topographic, climatic and geologic conditions.

It has been my observation that the mounds always occur upon areas of poorly drained sublevel surfaces in regions of abundant, periodic rainfall. They are also always underlain by formations of alluvial materials of relatively uncompact sands and clays.

The rainfall upon these places, owing to absence of well-defined runways, stands until it is evaporated or absorbed. The materials have different capacities for absorption and transmission, retention and loss of water, resulting in the unequal settling of the ground and the formation of the mounds and their interspaces. In the dry season these mounds are frequently augmented in size by drifting sands.

While writing upon this subject, I might add that last year I observed mounds exactly similar to those of the southern coastal plain region upon the Bavicora plain near the top of the Western Sierra Madre of Mexico at an altitude of nearly 7,000 feet. This plain, like the coast prairie, is an extensive flat upon which

the rainfall stands for a considerable time after falling.

Robt. T. Hill.

111 Broadway, New York,
April 20, 1906.

SPECIAL ARTICLES.

THE AVAILABILITY OF CELLULOID IN ILLUSTRATING CHROMATIC POLARIZATION.

1. It is not unusual to find that celluloid shows brilliant colors between crossed nicols on the cut edges. This observation suggested the use of the material to illustrate the properties of plates cut parallel to the optic axis, when seen in polarized light in the usual way. In fact, if a strip of celluloid is evenly stretched, fields of color vying in brilliancy with those of the natural crystal, may be obtained quite uniformly over an area an inch or more square, and variable at will through two or more well-defined orders; or the color of any given crystal may be similarly increased or decreased continuously in order. The well-known complicated figures seen in compressed or annealed glass are thus simplified, in a way that is at once interpretable in terms of elementary optical theory.

2. In the following experiment I used strips of clear celluloid, about 20 cm. long, 1 cm. or less broad (to avoid the need of excessive traction) and but .025 cm. thick (for flexibility). They were mounted between rollers, very much in the manner used in film cameras, except that one roller was rigidly fastened while the other could be rotated by the aid of a lever and clamped. If many strips are to be simultaneously stretched, it is advisable to secure one end of the strip under a plate of brass, bringing the ends around the remote edge and holding them down under a second smaller plate, in order that the maximum of friction may be encountered. The roller in this case is preferably a strong hollow brass cylinder (say 2 cm. in diameter) with a central longitudinal slot. Through this the ends are passed and wedged in place with a conical rod forced into the inside of the tube. About one complete turn should be taken to insure friction. For special purposes instanced below, a similar adjustment for stretching at right angles to the preceding should be added. Screw apparatus or uniform loads are also useful in particular cases.

3. As the two directions of vibration are parallel to the strain and at right angles to it, respectively, the nicols may be adjusted at 45 degrees to the vertical and the pulls be either horizontal or vertical. The phase difference ϕ for a thickness of strip, d, being

$$\phi = 2\pi d(n - n')\lambda,$$

where n and n' are the two indices of refraction for light of normal wave-length, λ. The plan of experiment consists in varying $n - n'$ continuously from 0 by increasing stress as far as the breaking point of the strip, and to increase d successively from the thickness of 1 to that of four strips ($d = .025$ to .1 cm.). The following results may be recorded:

One strip, $d = .025$ cm. Colors whitish to middle of the first order. When the strip breaks, the efficiency does not much exceed a quarter wave-length plate. Such a plate of mica, where mean and minimum elasticities are involved, is but $d = .0032$ cm. thick; a similar plate of selenite, where maximum and minimum elasticities occur, is .0027 cm. thick for mean wave-lengths. Hence the efficiency of a strip of celluloid stretched nearly to the breaking point is for like thicknesses, about 13 per cent. of that of mica and 10 per cent. of that of selenite.

Two strips, $d = .050$ cm. The earlier whitish colors now become more and more saturated and the strips break about at the end of the order.

Three strips carry the phenomenon (when stress is gradually increased) from colorless to the middle of the second order, extremely vivid colors overlying the whole visible area of the strip; four strips complete the first two series. If more strips are to be used the machine must be strong and quite perfect in its clutch, otherwise there is slipping and abrasion, by which the strength of the strips is decreased. Apart from this the experiments may be carried into higher orders of color at pleasure.

4. *Special Experiments.*—If three or four strips of successively decreasing width overlie each other symmetrically, so as to form a

steplike double or single wedge in cross section, the appearance is very striking, each thickness glowing with its particular color.

To produce a continuous wedge effect, the width of the strips should be gradually decreased to the center of the field. Since stress is greatest where the cross-section is least, the higher orders of colors appear nearer the center.

5. *Compensation Effects.*—If a plate quartz, cut parallel to the axis and showing the warm red between crossed nicols, is placed with its axis parallel to the lines of strain of the celluloid strip, the succession of colors is red, orange, yellow, green, blue or retrograde. The quartz effect is thus gradually more and more neutralized by the celluloid strip as its strain increases. If the axis of the quartz plate is at right angles to the lines of strain, the order is red, purple, blue, green, yellow or direct. The quartz and celluloid effect coincide in sign. Hence the ray vibrating in the direction of the lines of stress corresponds to the ordinary ray in quartz, which from the positive character of the wave surface, is the swifter. It follows that the ray vibrating parallel to the lines of stress of the celluloid strip moves with greater velocity than the ray vibrating normally to this direction. In other words the extraordinary ray is swifter and the wave surface of strained celluloid is negative: for on inclining the celluloid strip around the lines of stress as an axis, the succession of colors (due to increasing thickness) is direct in order; whereas on inclining the strip around an axis normal to the lines of stress the succession is actually retrograde, showing that the optic axis is being rapidly approached, where double refraction ceases in spite of thickness.

Similarly if a quarter wave plate of mica is inserted with the effective axis parallel to the lines of stress, the order of colors for crossed nicols is bluish, dark (neutral, compensation), bluish, yellowish, etc. (excessive celluloid effect), or clearly retrograde. If inserted with the axis normal to the lines of stress the succession of color is bluish, yellowish, red, purple, blue, etc., or direct, all of which admits of the same interpretation as in the case of quartz.

An equally interesting compensation is obtained by crossing two similarly stretched strips of celluloid at right angles. In this case the area where the strips overlap is quite neutral (dark between crossed nicols) while the four non-duplicated areas, extending outward from the square center, are vividly colored. The faster ray in one strip becomes the slower in the other, and *vice versa.*

My thanks are due to Professor Barus, who suggested these experiments, for his aid throughout the whole course of the work.

LULU B. JOSLIN.

BROWN UNIVERSITY.

AMŒBA BLATTÆ AND AMŒBOID MOTION.

THE writer wishes to call the attention of teachers of biology to a form of *Amœba* that hitherto has been somewhat neglected in this country, but which is of much theoretical interest. It is, moreover, sufficiently plentiful and easily obtained at all seasons of the year to be adapted to the uses of small laboratory classes. This is *Amœba blattæ* Bütschli, which inhabits the intestine of the croton bug, or common cockroach, *Blatta* (*Phyllodromia*) *germanica,* a well-known immigrant from Europe that has established itself in our larger towns and, at least in the eastern states, in many country villages. Throughout the year this cockroach is active in bake-shops, creameries, sugar refineries and in the kitchens and basements of hotels, restaurants and private houses, where it may be found under sinks, about water pipes and in similar warm, dark places.

Rhumbler,[1] in a recent paper in the 'Festschrift' commemorating Professor Ernst Ehler's seventieth anniversary, describes fully the movements of this *Amœba,* as well as the methods that have been employed at Göttingen for obtaining it. The cockroaches are etherized, the heads and terminal segments of the abdomen clipped off, and the intestine care-

[1] Rhumbler, L., '05, 'Zur Theorie der Oberflächenkräfte der Amöben,' *Z. f. wiss. Zool.,* 83 Bd., pp. 1–52.

fully pulled out with a pair of fine forceps into a few drops of one-half-per-cent. solution of common salt upon a glass slide. The intestine is then opened, and the contents scraped out and examined.

A preliminary attempt by the writer and his assistants to use *Amœba blattæ* as a laboratory subject with a class of a hundred and forty beginners did not meet with success, but little difficulty has been experienced in demonstrating it to small groups of students. Usually at least one in every four or five of the cockroaches which we have examined this winter has proved to be abundantly infested with the amœba.

Amœba blattæ is a rapidly moving organism, without pseudopods, and is of especial interest from the fact, which Rhumbler has demonstrated and which I have been able repeatedly to confirm, that backward peripheral currents occur in the endoplasm in addition to the axial forward stream.

The currents resemble those of a drop of oil or of other non-living fluid, in which the surface tension is unequally distributed. Such a drop contains an axial current, which flows in the direction of a certain area upon its surface over which the surface tension is diminished, turns outward in front, and forms backward superficial currents like a fountain. Hence the term 'fountain currents' has been applied to this phenomenon. In *A. blattæ* the backward superficial currents usually do not come to rest, as in the simpler fountain currents, but unite at the rear and enter the central forward stream, establishing thus that which Rhumbler calls a 'fountain whirl' ('Fontänenwirbel'). It is on the existence of such currents as these in *Amœba* and *Pelomyxa* that Bütschli, Rhumbler and earlier writers have based the theory that amœboid movement is due to diminished tension over a certain area of the surface, since drops of non-living fluid, *e. g.*, oil or chloroform, submerged in water and acted upon in such wise that the tension over a certain area of the surface is suddenly diminished, flow in precisely the manner above described.

The reader will recall that Jennings,[2] in his recent paper on the 'Movements and Reactions of Amœba,' denies the existence of 'fountain currents.' His careful and long-continued studies were based, however, principally on *A. limax, A. proteus, A. angulata* and *A. verrucosa,* and did not include *A. blattæ* or members of the genus *Pelomyxa,* in which such currents have been found by other observers.

Locomotion in *Amœba verrucosa,* and in the other species which came under Jennings's observation, is a rolling forward, as of a bladder half filled with water, or of a bag half full of shot, that is pushed over a flat surface. In this case the entire endoplasm continually streams forward, and the ectoplasm rolls onward around the endoplasm. A particle of soot, caught upon the surface of the advancing *Amœba,* is carried forward until it reaches the anterior edge of the animal, then downward beneath the body, where it remains quiet until the *Amœba* has passed completely over it, and it lies beneath the posterior edge. It is then carried upward and again forward to the anterior margin.

Rhumbler concedes that this form of locomotion can not be due to inequality in ordinary surface tension, and attempts to explain it as the result of unequal pressure at different parts of the surface, which is brought about by the unequal contraction of the ectoplasm as the result of gelatinization. The endoplasm at the anterior end of an *Amœba* in motion, according to this view, meeting there less resistance than at other points from the pressure of the gelatinizing and contracting ectoplasm, tends to burst forth from its confining envelope, and to become transformed into ectoplasm. Elsewhere, particularly at the posterior end of the organism, the reverse is supposed to take place, viz., ectoplasm is converted into endoplasm. An explanation of the lessened resistance of the ectoplasm at the anterior margin is sought in the stretching of the outer layer in that region, brought about

[2] Jennings, H. S., '04, 'Contributions to the Study of the Behavior of lower Organisms,' Carnegie Institution, Washington.

by the friction produced by adhesion to the underlying surface, which acts in opposition to the forward pressure of the endoplasm.

This theory is based mainly upon the analogous behavior of drops of chloroform which creep or roll spontaneously over a thin coating of shellac, with which the bottom of a glass dish is covered. After the shellac is dry, water is placed in the dish, and drops of chloroform are introduced with a pipette. A drop, under such conditions, rolls spontaneously in the opposite direction from the side which first shrinks back from the surrounding shellac. The adhesion between one side of the drop of chloroform and the shellac results in dissolving away the latter, in the diminution of surface tension over this side of the drop, and a rolling motion in this direction. Thus the drop plows a path through the shellac, propelled onward by the diminution of surface tension at its forward margin, which in the drop is produced by adhesion to the shellac. The drop of chloroform soon contains shellac in solution which, under the influence of the surrounding water, stiffens into a 'gel,' the contraction of which is supposed gradually to replace surface tension, as the active agency which propels the drop. If the gelatinous coating is of unequal thickness and strength at different points, motion occurs in the direction of a thin, weak point, where the contents burst through the superficial envelope.

By this analogy Rhumbler finds support for the theory, long since put forth by Berthold, that amœboid motion, at least in the amœbæ without backward currents, depends upon a one-sided adhesion to the substratum, and also for his own idea that the rolling motion of such amœbæ is due to the centripetal pressure of the gelatinizing ectoplasm and the bursting forth of the endoplasm at the anterior end, which continues the adhesion to the substratum and forms new ectoplasm. The analogy illustrates the possibility also that a gradual transition may occur ontogenetically and phylogenetically between the method of locomotion in amœbæ with fountain currents and active interchange between endoplasm and ectoplasm, and that in others, like *A. verrucosa*, with 'gelatinous' ectoplasm, which move without backward currents. Such a transition would depend upon the degree of gelatinization that had been reached. This process might be supposed to take place in *A. blattæ*, for example, until the backward peripheral flow should cease entirely, and the gelatinizing ectoplasm should reverse the direction of its movement over its free surface, and turn forward.

It is evident that Rhumbler's theory rests largely on an analogy with the behavior of drops of non-living fluid under special conditions. Facts which indicate that a constant gelatinization and the opposite are actually taking place in amœbæ with rolling motions are extremely meager. It seems highly probable that *A. blattæ* moves in response to unequal surface tension; but Rhumbler's explanation of the more complicated (rolling) motions of other amœbæ, though a useful hypothesis, needs the support of more facts drawn from direct observation of the organism itself, before it can be proved.

Jennings describes the rolling movement as consisting of the outflow of a wave of protoplasm at the anterior end of the *Amœba*, which exerts a pull upon the upper surface, drawing it forward, while the hinder portion of the *Amœba* becomes released from the substratum and contracts. Thus the 'strong pull from in front and the slight contraction from behind' cause the posterior end, and the inner contents, to flow forward. Jennings, accordingly, places emphasis upon the pull from in front, and looks upon it as a more important factor in locomotion than the contraction of the ectoplasm at the rear.

Rhumbler's theory at this point departs from Jennings's interpretation of the facts. The supposed gelatinizing, or semigelatinous, covering of *Amœba*, according to Rhumbler, is constantly bursting at its anterior edge, which is attached to the substratum. Out of the breach the endoplasm flows forward, as the result of the contraction of the gelatinous covering of the upper and posterior surfaces. If these are the facts, there can be no strong superficial pull exerted from in front back-

ward such as Jennings describes, for, according to Rhumbler, the ectoplasm in front is stretched between the *vis a tergo* and the friction against the substratum, is weakened and broken. The upper ectoplasm with a broken front edge can hardly be imagined to pull strongly upon the body behind it.

It is not expedient in this article to go more fully into the facts connected with this familiar phenomenon, which appears to be by no means as simple an action as Rhumbler supposes, when he compares it to the rolling of a rubber tire by hand or to the creeping of a drop of chloroform over a shellac-covered surface. Both of the papers cited, however, deserve the careful attention of every teacher of biology who touches upon the subject of *Amœba* and amœboid motion, although the conclusions of neither writer can be accepted without some modification.

John H. Gerould.

A CULTURE MEDIUM FOR THE ZYGOSPORES OF MUCOR STOLONIFER.

In the first edition of his 'Methods in Plant Histology' Professor Chamberlain speaks of the zygosporic phase of *Mucor* as being 'rarely seen' and requests information of anyone obtaining it. In the recent edition of the same work he refers to the researches of Dr. Blakeslee and then gives directions for making cultures for the zygosporic stage. The method described is rather haphazard and the tone in which it is stated indicates that the results would be doubtful.

During the past three months the present writer has obtained the zygospores so frequently that he now feels confident of being able to secure them at any time within a week. *With proper conditions of moisture and temperature, success is apparently dependent only on the nature of the substratum.* The substratum used is corn muffin bread, made, according to the baker, after the following formula:

Corn meal	16 pounds.
Flour	3 pounds.
Lard	3 pounds.
Salt	½ pound.
Eggs	48
Sweet milk	3 gallons.
Baking powder	18 ounces.

Half a dozen crumbs of this bread of the size of a thimble in as many tumblers, will yield on the average four or five cultures producing zygospores in large numbers in from five to seven days. The atmosphere should be kept saturated, the temperature about 70° F. and darkness is favorable though not necessary.

A series of experiments have been made and others are now under way to determine more exactly the conditions of zygospore formation.

J. I. Hamaker.

College Park, Va.

THE EFFECT OF FERTILIZERS ON THE REACTION OF SOILS.[1]

The effect of fertilizers on the reaction of the soil has interested both the farmer and the scientist for many years, but little experimental work appears to have been done on the problem, however. It is frequently held by farmers that the continued use of fertilizers, particularly of acid phosphates, and also potash salts and ammonium sulphate results in the failure of the red clover crop, a result which is attributed to the acid residues left in the soil by the selective action of plants in removing the essential elements from the salts in which they are applied. While there can be no doubt that certain fertilizing materials, notably ammonium sulphate, will produce an injurious degree of acidity, even changing the reaction of an alkaline soil, the evidence with regard to other fertilizers is not so positive.

Only recently have methods giving definite results been devised by which the total acidity of a soil may be determined. It is possible to determine the acidity of soil within practical limits by the lime-water method,[2] and I have determined the present acidity of a known naturally acid soil which has received different fertilizing treatment, by this method.

Dr. Thorne, of the Ohio Experiment Sta-

[1] Published by permission of the Secretary of Agriculture.

[2] *Jour. Amer. Chem. Soc.*, **26** (1904), 637.

tion, has very kindly sent me samples from a number of the plots of the five years' rotation experiments at Wooster, Ohio, where fertilizing experiments on limed and unlimed land are being conducted. The fertilizing experiments were begun in 1894, while the lime experiments were begun with the corn crop of 1900, each of the five series of plots being given 2,000 pounds per acre of lime (CaO) as it was prepared for corn.

The lime was applied to the plowed ground and harrowed in before planting corn, and the plots were again plowed before the sowing of clover which was sowed in the wheat and timothy a year later.

The reaction of a number of the plots, with other data, is given in the following table:

Plot. No.	Lime Treatment.	Total Fertilizer Applied in Two Rotations.	Acidity in Parts per Million.	
			Limewater Method.	Modified Sodium Chlorid Method.
2	No CaO	640 lbs. acid phosphate.	1,000	224
3	" "	520 lbs. potassium chlorid.	1,100	224
3	CaO	" " " "	Alkaline.	Alkaline.
4	No CaO	No fertilizer.	1,100	308
4	CaO	" "	Alkaline.	—
5	No CaO	960 lbs. sodium nitrate.	800	168
8	" "	640 lbs. acid phosphate, 520 lbs. potassium chlorid.	1,100	308
8	CaO	" " " " " " " "	300	56
12	No CaO	640 lbs. acid phosphate, 520 lbs. potassium chlorid, 1,440 lbs. sodium nitrate.	800	280
17	" "	960 lbs. acid phosphate, 520 lbs. potassium chlorid, 480 lbs. sodium nitrate.	1,100	280
18	" "	32 tons barnyard manure.	1,000	364
19	" "	No fertilizer.	900	364
19	CaO	" "	500	56
24	No CaO / CaO	960 lbs. acid phosphate, 520 lbs. potassium chlorid, 360 lbs. ammonium sulphate.	1,400 / Alkaline.	
29	No CaO	390 lbs. basic slag, 520 lbs. potassium chlorid, 960 lbs. sodium nitrate.	700	102

No difference could be detected in the reaction of water extracts of these soils; all were practically neutral.

For comparison, results by a modification of the sodium chlorid method[1] are also given. The modified method consists of treating 20 grams of the soil with 200 c.c. of $N/5$ neutral sodium chlorid solution in a Jena flask, allowing it to stand over night, filtering, boiling to one half volume and titrating with $N/10$ alkali. The results by this procedure are about double those by the regular sodium chlorid method with this soil.

The results here submitted have considerable interest. Bearing in mind the difficulty of securing representative soil samples, remembering that the method will not give results closer than fifty parts per million, it is still quite evident that several of the fertilizers have had considerable effect on the reaction of the soil. This is most evident on the plots that have received large quantities of sodium nitrate alone, or with other materials. In these cases the natural acidity of the unlimed plots has been materially reduced. Smaller applications of nitrate with full rations of acid phosphate and of muriate of potash have had no apparent effect. Potassium chlorid has not increased the apparent acidity despite the fact that the residue left by the salt is an acid, and the further fact that by double decomposition and absorption such salts give rise to acid reacting salts. Manure too has been without effect. Acid phosphate has, as it should when properly made, slightly reduced acidity. Practically the same story is told by the sodium chlorid results, plots 5 and 29 showing considerable reduction in acidity. An interesting point here is that although enough soda and lime have been applied to these plots to make them alkaline

[1] Bull. No. 73 Bureau of Chemistry, U. S. Dept. of Agr.

by this method the acidity has only been reduced about half. On the other hand, the acidity of plot 24 to which ammonium sulphate has been applied is materially increased, but Dr. Thorne writes me that the red clover grown on this plot is not visibly less than that grown on nearby plots to which nitrogen in linseed meal or dried blood was applied.

No explanation can be offered at this time of the behavior to both methods of plots 8 and 19 limed.

It appears then that while sodium nitrate and basic slag have diminished acidity, no fertilizer or combination of the fertilizers used has measurably increased acidity on this soil except where ammonium sulphate was applied. We can not apply this conclusion, however, to soils of different character. While the acidity due to the residue left by the taking up of plant food may reasonably be supposed to be irrespective of the nature of the soil, the acidity produced by decomposition reactions between the soil components and added salts is not. While in this soil the attack of neutral salt solutions upon what I have elsewhere called 'non-acid silicates' is small, with other soils it is very great, rising to 4,000 parts per million, and this fact must be kept in mind in attempting to measure the changes in soil reaction caused by the use of fertilizers.

F. P. Veitch.

CARBONATED MILK.

In the course of an investigation relating to the chemistry of kumiss made from cows' milk, the question arose as to whether there is any action of carbon dioxide on milk-casein. No action appears to take place when carbon dioxide is passed through milk simply at atmospheric pressure; but, since in kumiss the gas is present under considerable pressure, it was decided to approximate this condition by treating fresh milk with carbon dioxide gas under pressure. Without stating here the detailed results of the work, it was noticed that the milk thus treated did not sour or curdle readily, keeping ten days to two weeks at a temperature of 60° to 70° F., when the pressure used was sixty to seventy pounds. Pasteurized milk keeps still longer. In addition to prolonging its keeping power, milk, when carbonated, makes a very palatable, refreshing beverage. Before the detailed results are published, further work is being done, carbonating the milk at higher pressure and keeping it at different temperatures.

L. L. Van Slyke,
A. W. Bosworth.
New York Agricultural Experiment Station, Geneva, N. Y.,
April 16, 1906.

NOTES ON ORGANIC CHEMISTRY.

PREPARATION OF PURE ETHYL ALCOHOL BY MEANS OF METALLIC CALCIUM.

Metallic calcium having now become a regular article of commerce, several chemists have investigated its properties, in order to discover what advantages are likely to result from its use in chemical reactions. For example, in the chemical laboratory of the Johns Hopkins University, experiments are in progress to determine how far it may be of service in promoting the condensation of ketones with esters (Claisen's reaction).

L. W. Winkler[1] has examined its behavior towards mixtures of alcohol and water. As is well known, there is no special difficulty in removing water from alcohol—say by means of quick lime and copper sulphate—until it is 99.9 per cent. pure, but the elimination of the last 0.1 per cent. of water has been attended, hitherto, with considerable labor. By Winkler's process commercial 'absolute' alcohol, containing usually several per cent. of water, is boiled for a short time with calcium and then distilled from it. About 20 grams of the metal, in the form of turnings, to each liter of alcohol should be used. The product contains only 0.1 per cent. of water, which is removed by another treatment with calcium, in the proportion of 0.5 per cent. of the weight of alcohol. A curious point about the behavior of calcium and alcohol is that, if the latter contains less than 5 per cent. of water, the metal is attacked the more vigorously the *less* water is present, but, on the other hand, ordinary alcohol, containing more than 5 per

[1] *Ber. d. Chem. Ges.*, **38**, 3,612.

cent. of water, also attacks calcium with considerable rapidity. Alcohol absolutely free from water is not nearly so hygroscopic as is usually supposed; when 200 c.c. of it were exposed in a beaker to the air of a laboratory, during fifteen minutes, less than 0.1 per cent. of water was absorbed.

Almost all commercial alcohol contains aldehyde in varying proportion. The usual method of removing it is to boil the alcohol with potassium hydroxide until the aldehyde is converted into a colored resin, and then fractionate the liquid, the process being repeated, if necessary. Obviously, this plan involves a great expenditure of time and material and yet aldehyde-free alcohol is often needed, for example, it is indispensable in the analysis of fats and oils. Winkler removes the aldehyde by adding to the alcohol a little dry silver oxide, and then a small quantity of potassium hydroxide. The aldehyde is oxidized to acetic acid and this is neutralized by the alkali. F. L. Dunlap,[2] in a paper which followed Winkler's, suggests that the silver oxide should be formed *in* the alcohol. This is accomplished by dissolving silver nitrate in a very little water, mixing the solution with the alcohol to be purified, then adding, without shaking, cold alcoholic solution of potassium hydroxide. In this manner a finely divided precipitate of silver oxide is obtained which, in the course of a few hours, completely oxidizes the aldehyde. The alcohol may be separated from the silver compounds by decantation or distillation; it gives no color with potassium hydroxide.

NOTES ON ESTERIFICATION.

THE constant use in the laboratory of the esters of organic acids renders any improvements in their methods of preparation a matter of considerable general interest. Two papers on the subject containing results of some importance, have been published recently. In the first A. Bogojawlensky and J. Narbutt[1] record their experiments made to test the action of various dehydrated metallic sulphates on mixtures of alcohol and certain organic acids. Of the nine sulphates tried, potassium pyrosulphate and copper sulphate were found to be of the most service and, consequently, the majority of the experiments were made with them. The former salt acts equally well with both aliphatic and aromatic acids, the latter one is best suited for work with the aliphatic acids.

One of the most interesting results brought out in the paper is the relatively great effect on the yield of ester, of even small quantities of free sulphuric acid or anhydride, such as are almost always formed when a metallic sulphate is dehydrated. This is illustrated by the fact that in an experiment using ordinary dehydrated ferrous sulphate, the yield of ethyl succinate was 85 per cent., whereas, when the salt, before use, was repeatedly extracted with absolute alcohol, so as to remove traces of sulphuric anhydride, the yield of ester was only 34 per cent.

In general, the best yield of esters was obtained by the use of a dehydrated salt to which about 3 per cent. of its weight of concentrated sulphuric acid had been added; next in order comes the use of the dehydrated salt alone and, finally, sulphuric acid alone, as in the ordinary procedure. Another interesting point investigated was to see if a dehydrating agent, which is not acid and can not yield an acid under the conditions of experiment, is, nevertheless, capable of affecting the esterification. They selected the zeolite chabasite for this purpose, but found it to be without apparent influence on the course of the reaction.

The method here sketched offers several other advantages besides the question of yield. It can be applied to substances which are decomposed by concentrated sulphuric acid, and the ester, when formed, may be removed by simply pouring from the salt, without the rather tedious process of neutralization being necessary.

The esterification reaction has been investigated in another direction by J. Wade.[2] He finds that the process may be made continuous,

[2] *J. Amer. Chem. Soc.*, **28**, 395 (1906).

[1] *Ber. d. Chem. Ges.*, **38**, 3344.

[2] *J. Chem. Soc.*, **87**, 1656.

just as in the ordinary method for the preparation of ether. The essential condition for success consists in maintaining the mixture under experiment at a temperature of 100°, thus quickly removing the water which is continuously formed in the reaction. The directions for the preparation of ethyl acetate will serve to illustrate Wade's process: Three volumes of alcohol are mixed with two volumes of acetic acid, and two volumes of this mixture are added to one volume sulphuric acid, in an Erlenmeyer flask which is *immersed in* a water-bath. As soon as distillation commences, more of the mixture of alcohol and acetic acid is added by means of a funnel with a fine stem. Most of the excess of alcohol is recovered from the distillate. The process may be interrupted at any time without detriment, and there is no delay in restarting once the materials have regained the necessary temperature.

In the case of esters having boiling points above 100° the operation is conducted under reduced pressure. The presence of a strong mineral acid is essential to the success of the process, but more than a small proportion is detrimental. Charring seldom takes place.

J. Bishop Tingle.
Johns Hopkins University.

CURRENT NOTES ON METEOROLOGY.

MONTHLY WEATHER REVIEW.

The number of journals devoted to the different branches of science has become so large that most men of science can with difficulty, if at all, keep up with the periodical literature of their specialties. Yet there are none of us who do not frequently, or at any rate occasionally, wish to refer to some note or article, published in some journal devoted to another science than our own. It is convenient, for that reason, to have brought to our attention from time to time the more important articles, or at least the articles of most general interest, which are appearing in the various scientific periodicals of the world. It is with this feeling in mind that the compiler of these 'Current Notes on Meteorology' attempts to point out, from time to time, what there is of interest to scientific men in the meteorological publications of the various countries. It is impossible to devote much space in Science to these notes, for they obviously concern primarily only a single subject. But they may, perhaps, serve to help a fellow scientist, now and then, to learn of some meteorological publication which he has not seen or heard of, and which he may, at some time, find useful in his own work.

The *Monthly Weather Review* of our own Weather Bureau becomes more valuable with each succeeding year, as a meteorological journal covering a wide field, and essentially of a 'popular' nature. So prominent has the portion of the *Review* devoted to articles and notes become that with the first number for 1906, these articles occupy the first pages, instead of following, as they have done, summaries of climate and crop conditions, and accounts of the forecasts and storm warnings of the month. The last three numbers of the *Review* (November, December, 1905, January, 1906) contain the following articles of general interest:

'The Rainfall of China and Korea,' by T. Okada; reprinted from the Journal of the Meteorological Society of Japan; an important study of the climatic conditions of a region which is analogous in many respects to the eastern coast of North America.

'The Development of Meteorology in Australia,' by Andrew Noble; prepared at the request of Professor Cleveland Abbe, under the direction of the acting meteorologist of New South Wales.

'Indian Summer,' a note in which the sound and sane statement is made: "Indian summer is an extremely indefinite season as to its date and its character. There has never been any determination of its average date and duration so far as we know. It is often described as a warm, dry, hazy period after the first severe frost in autumn, but it often fails to come at all."

'A Mistake about Atmospheric Dust'; commenting on a statement which is going the rounds of the newspapers to the effect that 'rays of light go straight through all kinds

of gases,' and calling attention to refraction, the importance of dust and of aqueous vapor in coloring the sky, etc.

'Air and Water Temperatures,' by W. F. Cooper; a study of the effect of the water temperatures of Lake Michigan.

'The Climate of Madison, Wis.,' by James L. Bartlett, observer, Weather Bureau; a good general account of the climate of an important city, with some reference to the weather controls and weather indications.

'Tornado Insurance,' by H. E. Simpson, instructor in geology, Colby College; a paper written as a thesis in the course in General Climatology in Harvard University, and containing a number of facts not previously compiled from the present point of view.

'Meteorology in India,' containing notes from Mr. Gilbert T. Walker, now in charge of the Indian Meteorological Service. Of Mr. Walker's seven monsoon forecasts, six have been right, and one negative.

'Meteorological Maps for School Use'; it is a satisfaction to note that the Weather Bureau now supplies blank outline maps of the United States, suitable for laboratory work, at $2.50 or $5 per thousand, the price depending on the quality of paper used.

'Asymmetric Cyclones and Anticyclones in Europe and America,' by Professor F. H. Bigelow; the conclusions are somewhat startling, to wit:

> There is no evidence of the superposition of cold-center cyclones upon warm-center cyclones, as expounded by Clayton or by Bjerknes and Arrhenius, nor are there purely dynamic vortices in a rapid stream as supposed by Hann, nor are there cyclonic vortices caused by atmospheric islands of high pressure obstructing a rapidly flowing eastward drift as explained by Shaw, or by Hildebrandsson in his report to the international committee, 1905.

'Atmospheric Electricity,' by G. C. Simpson, the newly appointed lecturer in meteorology at the University of Manchester; deals with the latest aspects of the subject, chiefly in relation to meteorological problems.

'A Possible Extension of the Period of Weather Forecasts,' by Professor E. B. Garriott; calls attention to the value of a study of the great permanent areas of low and high pressure in making forecasts.

'The Relation of Forests to Rainfall,' by the late W. F. Hubbard; deals with the distribution of rainfall and forests in California, showing the close relation between the mean annual rainfall and the forest cover.

R. DEC. WARD.

REPORT OF THE ADVISORY BOARD OF THE WISTAR INSTITUTE.

THE advisory board of the Wistar Institute held its annual meeting in Philadelphia on Tuesday, April 17. The director's report of the year's work showed a decided step forward in the research work of the institute. The organization of the neurological work with Dr. Henry H. Donaldson as chief and Dr. George L. Streeter and Dr. S. Hatai as associates was reported. A statement was made of the financial condition of the institute so that the board might better consider the problems which might be undertaken. Following this report Professor Donaldson outlined the neurological work which he had under way and stated that some twenty pieces of research work were being actively pursued. Some of this work is already in press, some in manuscript, while a portion is being pursued in the laboratory of the University of Chicago and a portion in the laboratory of the Wistar Institute. Dr. Donaldson reported the action of the Imperial Academy of Science in Vienna in accepting the Wistar Institute as the central institute for brain investigation in the United States and appointing Drs. Donaldson, Mall and Minot as delegates to the meeting of the Central Committee for Inter-academic Brain Research to be held in Vienna this coming May. The advisory board considered the following question of policy: "With the understanding that all plans may be modified more or less from time to time to meet conditions as they arise, the question is presented for consideration: Shall we conduct the work of the institute after the manner of the usual research laboratories in the universities or shall we endeavor to make the work of the institute unique and try to do some of those things

which the university laboratories have been unable to accomplish." The general sentiment of the board was that the Wistar Institute should maintain a small staff of investigators of the highest type and expend a large portion of its income in maintaining artists, modelers and other mechanical aids to investigation so that there will be unexcelled opportunity in the institute laboratories for men from other laboratories to come and finish their researches in a much more satisfactory manner than it is possible to do in their own laboratories.

The question of furnishing material to investigators was discussed and it was decided that this plan should be pursued whenever possible. The neurological committee appointed last year was instructed to take steps for the further organization of neurological research in this country and it was suggested that, perhaps, a subcommittee of active neurologists should be organized in this country to meet occasionally and discuss the problems in their subject.

The question of organizing a pathological institute similar to that maintained in the state of New York was brought before the board and its various phases discussed. A committee of three was appointed to consider the possibility of such an institution. This committee consisted of Drs. Donaldson, LeConte and Piersol.

A resolution was passed suggesting that the institute collect research material for the researches in comparative anatomy and embryology whenever opportunity presents itself.

THE EARTHQUAKE AT STANFORD UNIVERSITY.

The injuries to Stanford University by earthquake of April 18 are, in brief, as follows:

1. Wreckage of the Memorial Church by the fall of the heavy spire, which crashed through the nave, the air blowing off the upper part of both ends of the church. The walls generally, of steel construction, are intact, but the building is ruined.

2. Wreck of the unfinished library. The great dome and its steel supports are unharmed, but their swaying completely wrecked the rest of the building.

3. Wreck of the new gymnasium, of brick faced stone.

4. Wreck of parts of the Art Museum which were made of brick faced with cement. The central part, of concrete strengthened by steel rods, is intact.

5. The Stanford residence in San Francisco, a huge wooden structure, heavily built, was not harmed by the earthquake, but is completely consumed by fire.

6. The inner quadrangle and engineering shops, of heavy masonry and one story high, are unharmed.

7. The outer quadrangle contains four large buildings reinforced by steel, the laboratories of zoology, botany and physiology, with the temporary library and the Assembly Hall. These are virtually unharmed.

8. The power house was wrecked by the tall stone chimney, which was snapped off like the lash of a whip.

9. The memorial arch had its upper part snapped off and is split almost to the base so that it is an entire wreck. This structure was of brick, reinforced with steel and faced with stone.

10. The chemistry building lost all its chimneys and is externally damaged by the fall of part of its stone facing. The building and its contents are little injured.

11. The four large buildings of the outer quadrangle, of brick unreinforced, and faced with stone, are somewhat damaged, the history building least, the incomplete mining building most.

12. Roble hall, women's dormitory, of concrete with steel wires, is absolutely unharmed except for the fall of two ornamental chimneys.

13. Encina hall, men's dormitory, a very large, finely built stone building, was injured by the fall of stone chimneys, one young man being killed. The building also has a serious crack in each of two corners, but is otherwise unharmed.

The wooden buildings on the grounds lost only chimneys and parts of plastering. No injury was done to books and very little to apparatus or collections. The working part

of the university as distinct from its architectural effects is little harmed. The most effective part of its architecture, the inner arcades with their Spanish arches and towers, is wholly undisturbed.

D. S. J.

REPORT OF THE STANFORD UNIVERSITY ENGINEERS ON THE INJURIES TO THE CLASS ROOMS AND LABORATORIES FROM THE EARTHQUAKE OF APRIL 18, 1906.

AFTER a careful examination of the buildings used for university purposes, including Roble and Encina Halls, we find that the damage from the recent earthquake is much less than was anticipated.

The buildings of the inner quadrangle, the one-story buildings of the outer quadrangle (with one exception), the zoology building, the physiology building, the assembly hall, the library, the old engineering building, the machine shop, the foundry and the mechanical laboratory are substantially intact and can be used after a few minor repairs.

The four corner two-story buildings and the small one-story physics building of the outer quadrangle, the forge shop, the woodworking shop and the chemistry building will require partial rebuilding of some of the walls.

In Encina Hall the south walls of the east and west wings will require partial reconstruction, also those portions injured by the two falling stone chimneys. The fall of one of the chimneys, which tore through the floors to the basement, caused the death of one student. Aside from the damage noted above the building as a whole is uninjured and perfectly safe.

The damage to Roble Hall is confined to the two holes torn in the floors by the falling chimneys. The remainder of the building shows practically no evidence of having passed through an earthquake.

In the opinion of the committee, such of the buildings mentioned above as are necessary for carrying on the university work can easily be made ready for occupancy and safe use in time for the opening of the university on August 23.

Our full and detailed examination of the buildings from foundation to roof shows that the actual damage to their stability is less than might be inferred from external appearances.

Signed by the committee:

CHARLES B. WING, structural engineer,
WILLIAM F. DURAND, mechanical engineer,
ARTHUR B. CLARK, architect,
CHARLES E. HODGES, architect,
CHARLES D. MARX, civil engineer, *ch'man.*

SCIENTIFIC NOTES AND NEWS.

AT the Washington meeting of the American Physical Society it was decided to hold the next meeting in Ithaca, N. Y., in conjunction with the meeting there of the American Association for the Advancement of Science, beginning on June 29. At this time also the new physical laboratory of Cornell University, which has just been completed, will be formally dedicated.

THE additional appropriation of $5,000 for the agricultural experiment stations, provided by the Adams bill, has now been paid. This bill increased the present appropriation of the agricultural stations under the Hatch and Morrill acts by $5,000 for the year ending next June, and by an additional $2,000 annually above the amount of the preceding year for the next five years. At the end of the five years this will amount to an increase of $15,000, bringing the total appropriation to each experiment station to $30,000 annually. The funds are to be applied only to paying the necessary expenses of conducting original researches or experiments bearing directly on the agricultural industry of the United States, with due regard to the varying conditions and needs of the states in which the stations are located.

THE *Osservatore Romano* officially announces that the Rev. John George Hagen, director of the observatory at Georgetown University, is in Rome and will be appointed director of the Vatican Observatory.

PROFESSOR FLORIAN CAJORI, dean of the Engineering School and professor of mathematics in Colorado College, has recently been elected a member of the Italian Mathematical Society —Circolo Matematico di Palermo. Professor Cajori has about finished his researches on

the history of arithmetic, algebra, theory of equations and theory of numbers from the period 1759 to 1799, and the results will soon be published in Volume IV. of Professor Cantor's 'Vorlesungen über Geschichte der Mathematik.'

In honor of Professor Trendelenburg, of Leipzig, the German Medical Society arranged a special meeting at the Academy of Medicine, New York City, on April 30. Professor Trendelenburg delivered an address.

Mr. A. Lawrence Rotch, director of the Blue Hill Meteorological Observatory, has commenced a fourth series of experiments with *ballons-sondes* at St. Louis, from which it is hoped to determine the seasonal change of temperature at great heights in the free air. This investigation is being conducted with a grant from the Hodgkins fund, held by the Smithsonian Institution.

Dr. Frederick G. Novy, professor of bacteriology in the University of Michigan, and Professor A. Laveran, of the Pasteur Institute, Paris, are the official speakers on the subject 'Preventive Inoculations against Protozoal Diseases' before the Section on Pathology and Bacteriology of the Lisbon International Congress of Medicine.

Mr. D. Brainard Spooner, a graduate of Stanford University, now in Leipzig, has been appointed by the British government director of archeological investigations in northwest India.

Dr. C. K. Edmunds, professor of physics and electrical engineering, Christian College, Macao, China, made magnetic observations during the winter vacation under the auspices of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington at ten stations in southeastern China and on the Island of Hainan.

Mr. Edgar R. Waite, zoologist in the Australian Museum at Sydney, has been appointed curator of the Canterbury Museum at Christchurch, New Zealand.

A message from Dr. Sven Hedin, the Swedish explorer, dated April 10, announces his arrival in Seistan after an extremely interesting journey *via* Jandak, Turoot, Khur, Tabbas, Naiband and Neh, in the course of which he crossed the Dasht-i-Kavir, the Great Salt Desert, three times. The explorer says he is in splendid health, and has collected material for a great work on eastern Persia. He has made a map of 162 sheets, has taken hundreds of photographs and sketches, and has formed a collection of specimens of rocks.

Professor Hugo de Vries delivered last week the third Spencer Trask lecture at the New York City College, his subject being 'Association of Characters in Plant Breeding.'

Professor Ernest Rutherford, of McGill University, is to deliver the following lectures at the University of Illinois: April 30, 'Cathode and Röntgen Rays'; May 1, 'Electronic Theory of Matter'; May 2, 'Radioactive Substances and their Radiations'; May 3, 'Passage of Electricity through Gases'; May 4, 'Radium and its Transformations'; May 5, 'Radioactive State of the Earth and Atmosphere.'

We regret to record the death, on April 13, of Walter F. R. Weldon, F.R.S., Linacre professor of comparative anatomy at Oxford University, and well known for his zoological researches, at the age of forty-five years.

The United States Civil Service Commission announces examinations as follows: On May 16, to fill such vacancies as may occur in the positions of laboratory assistant, assistant physicist, and assistant chemist in the Bureau of Standards, at salaries varying from $900 to $1,600 per annum, and vacancies as they may occur in any branch of the service requiring similar qualifications. On June 6, in the position of assistant in the laboratory, $1,200 per annum, Bureau of the Mint, Treasury Department, and similar vacancies as they may occur. On June 6–7, six vacancies in the position of aid in the Coast and Geodetic Survey, at $720 per annum each, and similar vacancies as they may occur in that survey. As the commission has experienced considerable difficulty in securing suitable candidates to meet the needs of the service, qualified persons are urged to enter this examination.

THE laboratories of the Rockefeller Institute for Medical Research at Avenue A and East Sixty-sixth Street, New York City, will be opened on May 11, at 4 P.M. Addresses will be made by President Charles W. Eliot, of Harvard University; President Nicholas Murray Butler, of Columbia University; Dr. William H. Welch, president of the board of directors, and Dr. Luther E. Holt, secretary of the board of directors.

THERE will be a scientific exhibit at the Harvard Medical School in connection with the approaching Boston meeting of the American Medical Association. Those who are willing to contribute to this exhibit should write either to Dr. F. B. Wynn, Indianapolis, or to Dr. W. T. Councilman, Boston.

THE New York Section of the Society of Chemical Industry will devote the meeting of Friday evening, May 25, to a symposium on color photography, at which meeting any new facts or specimens as examples of this new method of illustrating can be presented by applying to the secretary, Dr. H. Schweitzer, 128 Duane Street, New York City, or to Dr. George F. Kunz, 405 Fifth Avenue.

Nature states that on behalf of the family of the late Professor Manuelli, of Modena, Herr T. Waitzfelder has presented the Munich Museum with an interesting collection, in which are some original pieces of apparatus used by Galvani and other Italian investigators, together with some pieces of alchemistic apparatus.

A *Congrès de l'Alliance française et des Sociétés de Géographie* will be held at Marseilles in the middle of next September, under the presidency of M. Charles Roux. The congress is to be held at the National Colonial Exhibition at Marseilles, which is open from April 15 to November 15.

ON April 17 the free alcohol bill was passed by the House of Representatives by a vote of 224 to 7.

THE State Cancer Laboratory, Buffalo, will receive this year $21,000 from the state, $3,000 of which will be devoted to meet the deficit of last year.

FOREIGN exchanges state that a sum of $150,000 has been voted for the building of the new observatory at Bergedorf, with a further sum of $75,000, spread over two years, for equipment.

ANNOUNCEMENTS of the work offered at the Lake Laboratory of the Ohio State University have recently been issued and show some decided additions to those previously available. The staff of instructors has been increased to seven, and includes besides the director Professor F. L. Landacre, O.S.U.; Dr. W. E. Kellicott, Barnard College, Columbia University; Professor L. B. Walton, Kenyon College; Professor Malcolm Stickney, Denison University; Professor E. L. Rice, Ohio Wesleyan University, and Mrs. Harriet Gilbert, Painesville High School. Courses in general zoology, embryology, entomology, ichthyology, ornithology, invertebrate morphology, experimental zoology, vertebrate comparative anatomy and research work in zoology, general botany, ecology and special courses in botany. The laboratory also offers free tables to investigators who are prepared to do independent work. The courses of instruction open on June 25 and close on August 3, but investigators may use the laboratory for additional time if desired. For detailed information as to courses or copies of the announcement, application should be made to the director, Professor Herbert Osborn, Columbus, Ohio.

Two parties consisting of 105 junior engineering students of the University of Wisconsin spent the Easter recess visiting a large number of the important electrical and machinery plants in the east and the west. The western party, consisting of seventy-five students, inspected Chicago and Milwaukee car shops, electric plants, street railway and lighting power stations, foundries, ship yards and telephone exchanges, and similar works at Lockport and Joliet. The eastern party of thirty made a tour of the electrical power stations at Niagara Falls and Buffalo; the Westinghouse electrical machine and air brake plant; the Nernst Electrical Lamp Co., Pittsburg; the Brooks Locomotive works, at

Dunkirk; and the Homestead and McKeesport iron mills and furnaces.

A RECENT paper by A. W. Rogers, director of the Geological Survey of Cape Colony, South Africa, on the 'Campbell Rand and Griquatown series in Hay' (Trans. Geol. Soc. S. Africa, IX., 1906, pp. 1–9) gives among other things an account of a newly discovered series of glacial beds in the Griquatown series. The boulders included have beautifully striated surfaces, and the deposit itself is characterized as a very well developed glacial till, often thoroughly indurated, so that the boulders can not be broken from it. This deposit is of more ancient date than the Permian Dwyka formation, and is probably of early Paleozoic time; thus adding another curious problem to South African geology.

UNIVERSITY AND EDUCATIONAL NEWS.

IT is proposed, in connection with the opening next September of the new laboratories of the Harvard Medical School in Boston, to inaugurate a new system of advanced instruction in the medical sciences, designed especially to meet the needs of those desiring to pursue any of these branches as a profession. The system includes the organization of a new department of comparative anatomy, which has been placed in charge of Dr. Charles S. Minot, whose appointment was recently announced in this journal. The instruction in this department will be confined for the present at least to electives for fourth-year medical students and for graduates. The position of instructor in comparative anatomy has been established with a salary of probably $1,200. Candidates for this appointment should apply to Dr. Charles S. Minot, Harvard Medical School. The instructor will have charge of animal dissection, and will be expected to carry on original research subject to the approval of the head of the laboratory. The field of research will not be prescribed. The investigations already planned are in the domain of the comparative morphology of vertebrates. The laboratory is large and will be well equipped and furnished with a good working library. There will be special facilities for work in comparative embryology.

THE board of trustees of the Connecticut Agricultural College has authorized the executive committee of the board to receive and accept the Edwin Gilbert bequest consisting of a farm of 350 acres at Georgetown, Conn., together with a fund of $60,000 for the maintenance of the farm. The tract of land is to be used for experimental purposes in connection with the work of the agricultural college, but it is not intended in any sense to establish a branch of the college at Georgetown.

MR. LE ROY ABRAMS, assistant curator of the division of plants, U. S. National Museum, has been appointed assistant professor of systematic botany at Stanford University. Dr. E. C. Franklin, associate professor, has been promoted to a full professorship of organic chemistry and Dr. H. S. Blichfeldt has been promoted to an associate professorship of mathematics.

DAVID SAMUEL SNEDDEN, associate professor of education in Leland Stanford Junior University, has been appointed adjunct professor of educational administration in Teachers College, Columbia University; Dr. Kate Gordon, associate professor of psychology in Mt. Holyoke College, has been appointed instructor in educational psychology, and Miss Jean Broadhurst, of the Trenton (N. J.) Normal School, instructor in nature study.

THE board of regents of the University of Nebraska has made the following promotions: G. R. Chatburn, professor of applied mechanics; E. L. Hinman, professor of philosophy; Benton Dales, associate professor of chemistry; J. H. Gain, associate professor of animal husbandry; R. A. Lyman, assistant professor of pharmaco-dynamics; L. W. Chase, assistant professor of farm mechanics; F. D. Barker, adjunct professor of zoology; G. A. Loveland, assistant professor of meteorology. Professor H. R. Smith has been appointed head of the department of animal husbandry; Professor T. L. Lyon has been made professor of agronomy in the Agricultural Experiment Station, and F. D. Heald, professor of agricultural botany.